
Reconciling Science and Society: A Critical Historicist Approach

By Stephen Kemp

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Abstract

This thesis criticises a pervasive dualism in the philosophy of social science, the division between natural science and society. It argues that analysis which relies on this division misrepresents both natural scientific investigation and other social activities. From a dualistic perspective, those activities that allow a successful interaction with the material world, typically associated with natural science, are held to have a non-social aspect. Their theories (or 'meanings') are said to have a ground outside of the historical development and change which is characteristic of other social practices. It is this ground which is held to explain the progressive character of science. Conversely, those activities that are seen as fully social in character are theorised as if they were not variably successful in the achievement of their goals. As an alternative to this division, a 'critical historicist' approach is developed, drawing on post-positivist philosophy of science. It is argued that all social activities, including scientific investigation, are constituted by meanings, develop historically (rather than having a non-historical foundation), and are variably successful in character.

This conception of social activity is then used to criticise existing philosophies of social science for their dualistic approach. Both anti-naturalistic approaches, represented by structuration theory, and naturalistic approaches, represented by realism, are considered. Structuration theory argues that natural science is distinct from other social practices because the latter involve issues of meaning not encountered in the former. This claim is challenged, and it is argued that issues of meaning are the same in natural science and other social activities. Although realism's analysis of social life is inspired by natural scientific investigation, it also distinguishes the properties of the two, suggesting that social activity involves both an 'objective' (scientific) aspect and a 'social' aspect. The thesis argues that this separation leads to analytical incoherence, and an indefensible conception of both 'science' and 'society'. It is suggested that the adoption of a critical historicist approach would remove this incoherence.

The thesis then turns to consider the sociological analysis of science as a fruitful area for examining conceptions of science and society. The first approach analysed is social constructionism, which attempts to subsume science under a notion of social activity that sees all social understandings as equally successful. It is argued that this produces a contradiction in the constructionist framework. Constructionism both dismisses issues of variable success and brings them back by employing the notion of 'interests'. The thesis then examines actor-network theory, which explicitly rejects the science/society duality. It is argued that the alternative it offers involves two problems. Firstly, although scientific investigation is located historically, changes in understanding are projected into the realm of ontology. This undermines the possibility of theoretical comparison. Secondly, actor-network theory explains disputes within science and other social activities by calling on an unlocated notion of conflict.

The conclusion reviews the arguments of the thesis to consider how understandings of science and society can be reconciled.

Declaration:

The research contained within this thesis is my own work.

Stephen Kemp

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Introduction

This thesis attempts to transcend a pervasive dualism within philosophical and sociological thought: the division between natural science and social activity. Proponents of this divide suggest that science and social life have different characteristics. Natural science is said to be oriented to successful relations with the material environment, with actors supporting the most successful theories available. Successful relations with the environment are attributed to features of science which are non-social in character, such as the direct apprehension of the material world by scientists. Conversely, it is argued that social life is not oriented to success, but is 'meaningful' and/or 'strategic' in character. Social understandings are not held because they allow successful interaction with the environment, but because they are meaningful to actors, or allow them to pursue their sectional interests, which may be in conflict with what would be most successful.

In this thesis I hope to show that the division of science and society results in analytical incoherence. Accounts that characterise science as non-social are ultimately indefensible, and lead to a reification of the forms of reasoning or empirical categories that they take to be constitutive of science's success. Likewise, accounts of social activity that exclude considerations of success are unable to coherently theorise actors' commitment to beliefs. To go beyond this problematic division, the thesis makes a double movement. In the first move scientific investigation is reconceptualised so that its successes are explicable in terms which do not require it to be non-social. The groundwork in this area has already been done by post-positivist philosophers of science, whose accounts of scientific investigation allow close parallels to be drawn with other forms of non-scientific activity. However, philosophers of science have missed the opportunity to make this connection, and generally defend a dichotomy between scientific reasoning and social influences. In the second move, existing conceptions of the social are challenged for their insistence that the social realm has a special logic which excludes issues of variable success. It is argued instead that social activity is best understood as a knowledgeable attempt to successfully achieve goals through interactions with the material environment. The success of this activity is variable, depending on the adequacy of the knowledge that is employed in it. The result of these shifts is that all human activities, including science, are said to be fully social in character, and yet subject to assessments of their success or otherwise.

Although philosophical and sociological conceptions of science and society are frequently dualistic, a key exception to this is the work of John Holmwood and Alexander Stewart. In *Explanation and Social Theory* (1991) Holmwood and Stewart argue that it is possible to integrate the analysis of science and society using post-positivist principles. They suggest that attempts to separate a specific social logic from the logic of successful relations with the material environment necessarily result in contradiction. Holmwood and Stewart are particularly concerned with the way in which this undermines the adequacy of sociological accounts that call on such social factors to explain human activity. As an alternative, they argue that sociological analysis must take into account the success or failure of social activities. This thesis takes its inspiration from the approach of Holmwood and Stewart, extending their work in two respects. Firstly, it offers a detailed analysis of the theories of Archer, Barnes, Bloor, and Latour, which are given only brief consideration by Holmwood and Stewart. Secondly, it draws out the implications of Holmwood and Stewart's work, exploring how a non-dualistic mode of analysis can improve our understanding of natural scientific investigation and other social practices.

The thesis begins, in Part I, Chapter 1, by considering recent developments within the philosophy of natural science, particularly in the work of Thomas Kuhn, Imre Lakatos, Larry Laudan and Dudley Shapere. Drawing on insights from these authors, I argue for a 'critical historicist' analysis of natural science that is compatible with its character as a social activity. Crucially, critical historicism rejects the notion that scientific investigation has an unshakeable foundation in experience or method that guarantees the truth or adequacy of its claims. Rather, both substantive categories (including those of experience) and methodological rules are held to change over time, placing scientific investigation in the flow of history. An important aspect of this non-foundational view is that scientific categories are no longer conceived to be in direct contact with the world. They are, instead, to be conceived of as 'theoretical' or 'meaningful' mediations which are more or less successful in ordering the world. On a critical historicist view, the truth of these categories cannot be inferred from their success, as further development may require their reconstruction to expand the capacity of the theory. However, the absence of a claim to truth does not undermine the nature of science as progressive or give rise to relativism as is frequently alleged.

Chapter 1 also briefly introduces how this analysis of science can be used to question attempts to divide it from other social practices. Social life is frequently differentiated from scientific investigation, on the basis that social practices are constituted by meanings.

However, the critical historicist view suggests that scientific investigation is constituted by theoretical mediations (i.e. meanings) that change over time. Both natural scientific investigation and other social practices are thus meaningful in character. This connection can then be turned back on existing notions of social activity to criticise them. In sociological theory, the meanings involved in social life are typically taken to be 'self-validating', that is, adequate on their own terms. For critical historicists, although theories are mediations and have no direct relation to the world, they may be more or less adequate, depending on how coherently they can account for interactions with the material world. This suggests a way to bring issues of success back into the analysis of social life while accepting that social institutions are historically variable and constituted by meanings. It also retains a parallel between natural science and other social activities as meaningfully constituted, and of variable adequacy.

The other way in which science and society are typically contrasted is the claim that scientific beliefs are held because of their generally accepted success, whereas in social life beliefs may be held because they are in the interests of a particular social group. Social life is argued to involve the strategic pursuit of interests in a way which natural science does not. Contrary to this, I suggest that 'science' and 'interests' are not genuinely in tension with one another. Critical historicism argues that coherent beliefs allow a successful interaction with the environment to produce resources. As social interests are oriented to resources, their pursuit is not contrary to the pursuit of scientific knowledge, but calls on this to achieve desired outcomes.

These ideas are explored more fully in Parts II and III of the thesis. Part II addresses existing approaches to sociological theorising. I argue that although there appears to be a dispute within sociology over the nature of social science, and the extent to which it can be modelled on natural science, both naturalistic and anti-naturalistic approaches call upon a science/society dualism. I examine existing approaches to show that they fail to offer a coherent account of the relation between science and society, which ultimately undermines the coherence of their analyses. Chapter 2 discusses the anti-naturalist argument that social science and natural science are necessarily different in character. This is based on the claim that there are issues of 'meaning' arising in social life which do not occur in natural scientific investigation. For this chapter, I focus on the ideas of Anthony Giddens, as his work provides a useful compendium of anti-naturalistic arguments. I suggest that these arguments are unconvincing, and that issues of meaning are the same across natural scientific

investigation and other social activities, undermining the division between the two. As a consequence, natural scientific investigation provides a viable model for social science.

Chapter 3 addresses the realist approach to sociological theory. Realists argue for a qualified naturalism in relation to social science, portraying their analysis of society as scientifically inspired. This stance is used to distinguish realism from anti-naturalistic approaches. However, as with the latter, realism calls upon a dualism between science and society, suggesting that society has special properties differentiating it from science. I argue that this separation leads to theoretical incoherence, and show this by analysing the work of Margaret Archer, who is an important recent exponent of realism in the social sciences. Archer argues that the analysis of society must incorporate a consideration of the objective logic discovered through science, but have an additional 'social' component to capture the reflexive and strategic nature of human action. On this basis, Archer sets up a series of dualisms such as structure/agency, culture/agency and structure/culture which she insists are necessary to properly understand society. I examine these dualisms and argue that they cannot be used to produce a coherent analysis. The realist attempt to differentiate a 'social' component from the objective possibilities for successful action analysed by science cannot be sustained. Conversely, Archer's account of science is overly-objectivistic, and fails to recognize the historical and mediated character of scientific understandings.

Recent sociological theorizing calls on a science/society division, resulting in analytical incoherence. In Part III of the thesis, I turn to sociological analyses of natural science to see if these offer a way of transcending the dualism. This seems a potentially fruitful area to consider, giving that such theories bring together questions about the character of natural science and social activity. In Chapter 4 I consider constructionist analyses of science, focusing on the work of Barry Barnes and David Bloor, founders of the Strong Programme. Barnes and Bloor argue that natural scientific activity is fully social in character, a claim supported by this thesis. However, they do so by analysing natural science in a relativist fashion, arguing that all theories are equally adequate in their relations to the material environment. When actors claim that one theory is more adequate than another, this is to be explained by locating the social interests which they are pursuing.

This relativist approach takes the 'social' side of the natural science/society dualism and attempts to subsume science under it. The analytical difficulty that emerges is that social interests, which perform an important explanatory function, cannot be located in a successful relation to the material environment and the resources that accrue from this, because issues of success have been excised from analysis. I consider the problems this produces in some

of the examples analysed by the Strong Programmers. As an alternative I suggest that interests are intrinsically connected with the success of knowledge, breaking down the separation of science from society. I then move on to consider Barnes and Bloor's analysis of social institutions as self-referential and self-validating, which generalizes their approach to science and applies it to phenomena such as money and status groups. I argue that this attempt to analyse social institutions without incorporating issues of success suffers from the same difficulties as the relativist analysis of science. This suggests that the conception of social activity that emerges out of a science/society dichotomy is equally problematic whether it is applied to scientific investigation or to other social activities.

In Chapter 5 I consider actor-network theory, which offers a more thorough rejection of the science/society dualism than that of constructionism. Looking at the work of Bruno Latour, I explore the actor-network idea that actors attempt to order the world around them through a pragmatic process of definition, which, if successful, allows the actor to build up their resourcefulness. Actor-network theory challenges the science/society divide by suggesting that definitions of human and non-human actors should be treated in the same fashion, rather than the human (social) and non-human (natural) being analysed as separate realms each with its own autonomous logic. Latour puts this idea to good use in his critique of constructionist accounts of science, which shows the difficulties in locating a realm of purely 'social' interests, outside of human relations with nature. His alternative is to argue that scientific debates involve disagreements over the nature of both human and non-human actors, which can become redefined in the process.

I argue that Latour's account has two key problems. Firstly, although Latour correctly identifies scientific investigation as a transformative process, he suggests that it is ontology that is transformed and not just understandings of nature. This generates problems in connecting the analysis of past, present and future science, which exist, for Latour, as different ontological worlds, rather than being different representations of the same world. Secondly, Latour does not locate disputes over the nature of human and non-human actors in problems of ordering, but explains them by referring to the clash of opposed defining wills. Although this avoids the science/society dichotomy, it fails to locate conflict any more successfully than constructionist accounts of interests, or realist arguments about strategic action. As an alternative I suggest that explanations of conflict must locate it in problems of ordering the material world. The chapter concludes by considering Latour's criticisms of social science, which he sees as having 'legislative' pretensions to impose its own order on the world. I argue that this criticism is misplaced, and that social science can make

arguments about the success or otherwise of modes of ordering without therefore being legislative.

This brings us to the Conclusion, in which I review the arguments of the thesis to consider how understandings of science and society can be reconciled.

Part I

Science Reconsidered: New Philosophy of Science

Philosophy of Science: A Critical Historicist Perspective

1.1 Introduction

Recent developments in the philosophy of science have significantly altered our understanding of scientific activity. Writers including Thomas Kuhn, Mary Hesse, Imre Lakatos, Larry Laudan and Dudley Shapere have all offered theories of science that attempt to supersede those of their positivist predecessors. Although post-positivism can be dated back at least as far as Thomas Kuhn's *The Structure of Scientific Revolution* (1962), it seems fair to say that its ramifications are still being worked through. One reason for this is that although recent writers share a rejection of positivism, they frequently disagree with one another about the best alternative account of science (see for example Lakatos and Musgrave (1970), Doppelt (1986), Laudan (1987)).

In this chapter I want to reflect on what can be learned from post-positivist philosophy, and pull together various ideas from this current of thought to defend a 'critical historicist' conception of natural scientific investigation. This account is 'historicist' in that it suggests that scientific theories do not have solid foundations in empirical 'facts' or modes of reasoning which guarantee their adequacy. I argue that instead of being outside of the flow of history, both empirical concepts and theoretical standards change over time. Nevertheless, this lack of foundations does not mean that we must accept the relativist claim that all theories are equally valid. The account offered here is thus 'critical' in the sense that theories and standards can be assessed as to their adequacy. This assessment is an immanent one, based on the extent to which theories successfully account for interactions with the environment on their own terms.

I believe that the argument of this chapter has intrinsic interest as an attempt to reconstruct a viable account of science drawing on elements of post-positivist philosophy. However, it also provides the basis for addressing the central theme of this thesis, that is, the division between science and society. Both sociologists and philosophers have divided natural science from society on the basis that its success in interacting with the material world must be the result of its special epistemological characteristics. Scientific investigation is said to

have unquestionable foundations because of the direct contact of concepts with the empirical world, or because its modes of reasoning guarantee its success. Other activities are then separated from science on the basis of their different orientation to success. This chapter suggests that the foundational account of science is wrong, and argues that scientific theories have no special epistemological properties, but are mediations which provide more or less successful ways of interacting with the material world. By challenging the foundational account of science, this chapter destabilizes the division between science and society, and the rest of the thesis explores how other social activities can be directly paralleled with natural science once this is properly conceived.

The chapter proceeds as follows. Firstly, I discuss post-positivist criticisms of foundational conceptions of science, and use these as the basis of the critical historicist approach. I then compare this approach to other recent conceptions of science, namely epistemological and ontological realism. The remainder of the chapter explores why critical historicism's rejection of foundationalism need not lead to relativism.

1.2 Science as an activity without guarantees

To demonstrate the importance of post-positivist views of science, we must begin by considering foundational theories of scientific knowledge, and their weaknesses. One foundational approach is 'empiricism', which argues that theories are based upon observational facts that can be regarded as conclusively verified. Such a set of unquestionable empirical facts (accumulating as scientific investigation continues) would provide the basis for adjudicating between theories, and any theoretical disagreements could always be resolved by reference to existing observational facts, or those that might be accumulated, in order to solve a dispute. This kind of approach was defended most recently by the logical positivists, but their work has been subjected to heavy criticism (for a general discussion see Hanfling, 1981; Hesse, 1974). There are strong arguments to suggest that there can be no concepts resulting from investigation which possess the status of unchallengeable facts. By now these arguments are familiar and standard within the philosophy of science, but it is worth reviewing them in order to understand their consequences.

The idea that empirical claims can be conclusively verified is thrown into doubt by arguments asserting that observation is theory-laden. That is to say, some philosophers have opposed the notion that there can be 'direct' or 'unmediated' reports about the state of the world. Observation reports are always made in a language that is theoretical, and invokes

certain assumptions or claims about the world which are open to challenge from a competing perspective. Taking a common sense example, Alan Chalmers argues that a claim such as 'Look out, the wind is blowing the baby's pram over the cliff edge!' invokes a range of low-level theoretical assumptions, about the existence and character of 'wind', as well as implicit expectations about the outcome of the pram's journey, and so on (Chalmers, 1982: 28-9). When we move into the realm of scientific observation reports it becomes even more apparent that claims such as 'The electron beam was repelled by the North Pole of the magnet' cannot be taken as direct descriptions of a set of events in the world (Chalmers, 1982: 29).

The critical reader might argue that such examples involve assumptions that are all too clearly 'theory-laden', and that what is required is a move to more basic and solid empirical claims, such as 'Object X is red'. Perhaps if such claims could be conclusively established, then more complex constructions could be made from these solid building blocks. Mary Hesse considers this case in order to argue that even such an apparently basic claim is not independent of theoretical assumptions. Hesse argues that for the claim 'Object X is red' to be an observation report it must be intersubjectively verifiable, that is, open to confirmation by all competent observers (Hesse, 1974: 17-8). If this is so, however, then it is possible to suggest that an observer has made a mistake in predicating 'red' of an object. This would be done by making claims like: 'It can't have been red, because it was a sodium flame, and sodium flames are not red' (Hesse, 1974: 18). Such corrections do not make use of 'direct' experience as such, and in fact can be used to challenge the claim that a 'direct' apprehension of redness actually occurred in the first place. Thus, it seems plausible to say that the correct usage of the term 'red' is subject to a public debate about the conditions of its applicability, a debate that can invoke 'theoretical' assumptions about the connection of red to other predicates. This being the case, observational reports such as 'object X is red' cannot be taken as purely direct and unmediated.

To take this insight into the realm of science, Hesse examines a concept that appeared to be highly stable and conclusively verifiable in its status, that of time simultaneity (1974: 19-21). Hesse argues that before the theory of special relativity was developed, it was believed that judgements about the timing of events, that is, about their simultaneous occurrence or sequential ordering, were absolute and standard between all competent observers. Before 1905, such judgements would have been taken as paradigm cases of observational claims whose application had the status of empirical fact, and which were non-theoretical. However, Einstein argued that judgements about timing may indeed vary between differently

placed observers, if, for example, one observer is travelling at a constant velocity away from the other (Einstein, 1962). He also suggested, of course, that in certain cases¹ one can use the knowledge of the time and spatial location of events as judged from one standpoint to calculate the time and spatial location of events in another standpoint. This, as Hesse points out, does not save the notion of 'simultaneity' from theoretical assumptions (such as the constancy of the speed of light in a vacuum), but merely explicitly reformulates the assumptions that were already implicitly present in the Newtonian conception (Hesse, 1974: 20). Consequently, we have here an example of the vulnerability to theoretical criticism and reformulation of a previously stable 'observational' claim.

Another way of putting this is to emphasise the mutual implication of the 'positive' and 'residual' categories of a theoretical system. Following Talcott Parsons², John Holmwood suggests that we can conceive of a scientific theory as a structure of concepts which attempts to offer a coherent account of all the relevant phenomena of some domain, and the logic and development of its processes (Holmwood, 1996, especially Chpt. 3). When a successful and consistent account of a process has been given, the categories used can be regarded as 'positive'. Until a theoretical structure has been perfected, however, there will always be not only positive categories, but 'residual' ones as well, which are used to describe those 'facts known to exist' but which the theorist cannot give a systematic account of (Holmwood, 1996: 36). The important point about viewing theory development in this way is to emphasise the mutual implication of the positive and residual categories of a system.

For one thing, this means that those supposed 'facts' that contradict the existing theoretical statements will be transformed in character, as theoretical reconstruction necessitates their reinterpretation in order to make their character systematic (Holmwood, 1996: 37). One example of this is the theoretical development of Prout's hypothesis that all elements were composed of hydrogen, and thus that the atomic weight of each element should be divisible by a whole number into the weight of hydrogen (discussed in Laudan, 1977: 31). During the nineteenth century there were several well-known 'factual' anomalies to this doctrine, such as the atomic weights of 103.5 for lead, and 35.45 for chlorine. However, by the twentieth century, the theory of isotopes had been elaborated, and, after separating out isotopes of the same element, each isotope was found to have a whole number

¹ These are cases where one observer is not moving rotationally in relation to the other, and where one observer's motion is uniform (of constant velocity and direction) in relation to the other (Einstein, 1962: 12).

² Parsons develops this account to analyse the development of categories within sociological theory (Parsons, 1949).

atomic weight. Thus, the earlier 'facts', which were residual categories for Prout's theory, were transformed in their character and became positive elements of the theoretical system.

This example also illustrates the converse aspect of theoretical development, which is the transformation of positive categories in order to develop the applicability of the system. Holmwood writes that the process of development 'transforms the positively defined categories of previous statements of the theoretical system, as explanations are extended and new relationships postulated' (1996: 37). As we can see in relation to Prout's theory, the concept of a chemical element had to undergo what Lakatos calls a 'concept-stretching expansion' so that, unlike the older approaches, isotope theory postulated pure elements which behaved identically in chemical reactions, but could be separated by physical means (Lakatos, 1978: 54). Thus, both the positive and the residual categories of the theoretical system had to be reconstructed.

Of course, as Duhem argued, although we may be aware of a 'problem' in our understandings, that is, an outcome or event that we cannot systematically locate, this does not tell us which of our 'positive' categories will require alteration in order to remove the problem. It may well be that the 'problem' lies in quite some other body of knowledge than that which we are interested in developing. For example, we may argue that there is an error in the optical theory which is implicated in certain 'anomalous observations'. This is exactly what occurred in the development of Newton's work, in which he successfully challenged existing observational theories used to determine the position of the moon (Lakatos, 1978: 45-6, fn. 5). In this way, our own positive categories could be left intact by locating the problem elsewhere. More will need to be said about this, and the modes of developing theories in response to problems. The general point here, though, is that the 'positive' and 'residual' categories of a theoretical system are both 'mediations' that are subject to revision and development in the pursuit of improved understandings. Neither provides a direct apprehension of the objects and processes that we are interested in understanding.

We have considered so far that there are no guarantees to be found for science in either the positive observational reports and empirical facts or the anomalous 'falsifications' that are the results of investigation. This is not the only way in which one could argue for the certain foundations of science. Rather, it could be claimed that there are certain standards for judging the validity of competing theories which would provide conclusive grounds for preferring one theory over another. If these standards could be shown to have an unshakeable basis, then this would undermine the idea that science is an activity without certitude.

Theorists such as Larry Laudan and Alan Chalmers have questioned the notion that standards for theory choice provide unquestionable foundations for science. Firstly, they argue that such standards of validity have altered over time, and that this suggests their non-foundational character. Laudan offers the example here of the 'method of hypothesis' which holds that a hypothesis can be considered validated if all of its examined consequences are true (Laudan, 1981b, especially Chpt. 2). Laudan convincingly suggests that the status of this standard for judging the validity of theories has oscillated over time. In the seventeenth century Descartes supported the method of hypothesis, along with Boyle, Hooke, and others. Into the 18th century, the method fell from grace, with writers such as Voltaire, Priestley and Euler rejecting hypothetical reasoning in favour of strict induction. By the 19th century however, methodologists such as Comte and Whewell had reinstated the method of hypothesis as a viable mode of inference (Laudan, 1981b: 9-10). Likewise, we can see the disputes over whether a falsifying instance should lead to the rejection of a theory (as in an extreme version of falsification), or whether the prediction of novel facts provides support for a theory (as in Lakatos, Leplin etc.) as suggesting the insecurity of such rules, and their revisable nature. If these latter examples seem too abstracted from scientific practice, we can examine the clash between Descartes' standards for knowledge requiring it to be 'based on self-evident, a priori, first principles' and those of Newton's theory which in employing the notion of gravity as action at a distance was treated as highly mysterious (Chalmers, 1990: 20).

There is a sense in which the defender of a foundational notion of rationality has nothing to fear from these examples. After all, displaying the mere historical variability of standards of validity does not provide a direct challenge to the idea that undeniable, foundational standards could be located. However, there would seem to be little indication that the history of scientific practice, as we know it, has been underwritten by such standards, and there are a range of apparently important scientific episodes in which the standards employed to judge theoretical clashes were quite different.

It is important to identify precisely the parameters of what is being argued here. The issue at hand is whether unquestionable standards of judgement guarantee the theories produced by science. All that needs to be established is that there are no standards consistently operating across important episodes of scientific activity which can be relied upon to produce definitively warranted theory choices. As such, we can leave unquestioned Chalmers' claim that there is a general 'aim of science' which involves the push towards generality and the growth of knowledge, as he accepts that this can only provide 'rough

schematic guidelines' for methodology which will need to be worked out in practice themselves (Chalmers, 1990: 39). In other words, this provides nothing like a foundation that can guarantee the adequacy of scientific concepts.

This point should be born in mind when assessing Jarret Leplin's arguments about scientific standards. Leplin takes issue with Laudan's claims about the diversity of standards in science. He argues that there are 'general, sustained methodological and axiological themes that survive changes in the localised prescriptions and constraints that scientific discoveries introduce' (Leplin, 1990: 24). His list of such stable themes includes the following claims: that there are laws of nature; that nature obeys exact regularities; that natural laws can be given mathematical formulations; that greater precision in measurements provides greater evidential support; that experiments reveal regularities not apparent in experience; that science seeks truth and generality; that it counts empirical adequacy as a criterion of truthlikeness; and that hypotheses must be tested (Leplin, 1990: 24-5). Leplin's argument is that these consistent aspects of science are the 'central and guiding ones from which unstable ones are derivative' (Leplin, 1990: 25).

Does Leplin's argument suggest that there are standards that guarantee theory choices in science? Although arguing for the stability of certain features, Leplin accepts that there are many lower-level changes in scientific standards (1990: 21, 25). If this is the case, then scientific decisions based upon such standards cannot be considered to be foundationally justified. We may argue that scientific standards are being improved as science goes along, but the fallibility of judgements (demonstrated by previous changes) undermines the idea that scientific decisions are conclusively warranted. For example, if theories were at one time rejected because of their hypothetical character, the 'rational' justification of the method of hypothesis at a later date suggests that previous scientific decisions were taken using a misleading criterion. There seems to be no particular reason to think that debates around standards such as these have reached a conclusive end-point, and what is presently accepted as legitimate may later turn out to be rationally unwarranted.

As to Leplin's higher-level features of science, they appear to be too general to allow decisive judgements about particular issues of theory assessment (something that Leplin himself would probably accept). Furthermore, not all of these very general criteria can be treated as uncontroversial. After all, Bas van Fraassen (1989) has recently argued that the idea of a 'law of nature' can be brought into question, and quite what such 'laws' consist in is up for dispute between various positions (see arguments between realists and positivists for example). As Laudan suggests in his reply to Leplin, what appears on the surface as

commitment to the same standard may hide an underlying diversity which undermines the apparent 'stability' of some particular feature (Laudan, 1990: 49). A close examination of the idea that science is the search for 'truth' or that empirical adequacy is a criterion for 'truthlikeness' would likewise reveal a diversity of views on these subjects. These are good grounds for arguing that there is no fixed set of standards which guarantee the outcomes of scientific investigation.

This brings us to the end of the section, which has argued that scientific claims are not founded upon unquestionable observational concepts, or modes of reasoning that guarantee success. It should be noted that the former argument is less contentious than the latter, which is still resisted in some philosophical quarters. I hope enough has been said to make the argument against the certainty of scientific investigation somewhat plausible. As an alternative, the critical historicist approach views scientific theories as a set of mediated and fallible understandings produced through investigation. The methods by which these theories are produced, and the standards by which they are assessed, have no solid foundations, and change and develop along with more substantive matters during scientific debate.

1.3 Theoretical mediation and problems of understanding

It is important now to turn our attention to the issue of the 'mediation' of theoretical categories. I have argued above that an emphasis upon the mediated nature of all claims about the world has been a fundamental tenet of recent philosophy of science. Now it is time to examine carefully the consequences of 'mediation' in relation to natural scientific activity. In order to do so, we should turn to the work of three important post-positivist thinkers, Kuhn, Lakatos and Laudan³ to examine their accounts of natural scientific activity.

In the work of these thinkers, scientific investigation is necessarily based on an agreed set of theories, standards and/or exemplary problem-solutions which organise the activity of scientists. For Kuhn, this is a 'paradigm' which includes a variety of elements such as symbolic generalisations, metaphysical commitments, values, and exemplary pieces of

³ In his recent *Beyond Positivism and Relativism* (1996) Larry Laudan is highly critical of 'postpositivist' philosophers of science. The grounds for this criticism are that writers in this category (such as Kuhn and Feyerabend) respond to logical positivism in a way which does not escape its basic assumptions, leading them to relativist conclusions. For the purposes of this chapter, however, I will use the term 'post-positivism' to signify those recent positions in the philosophy of science which reject the split of theoretical and observational terms in favour of seeing all scientific understandings as mediations.

earlier science which provide a model for further problem-solving⁴ (Kuhn, 1970: 182-7). Kuhn argues that without the focusing of research that a shared paradigm allows there can be no properly scientific research. Without a shared paradigm, practitioners all approach investigation from different perspectives, using different principles and techniques, and the result tends to involve continuing debate around first principles rather than the successful development of understanding (Kuhn, 1970: 10-13).

Lakatos's account of scientific growth in terms of competing research programmes contains similar features. The research programme guiding scientific activity is constituted by basic methodological rules of two kinds, the positive and negative heuristic. The negative heuristic or 'hard core' defines which claims are taken to be central to the programme and to be defended at all costs from refutation, thus being allowed a certain distance from experimental and observational claims. Defence of this hard core provides a stable base to investigation from which further theoretical developments can be launched. These developments occur through the construction and development of a 'protective belt' of auxiliary hypotheses that are used to deflect apparent anomalies. Meanwhile, these anomalies are taken up and theorised in a relatively systematic way, as defined by the positive heuristic, in order to develop the scope and applicability of the programme (Lakatos, 1978: 47-52).

For Laudan, the key to understanding science is that it is a problem-solving activity. However, the problems addressed by science are not directly given, and only arise within a certain stage of inquiry, that is, a specific theoretical context. Laudan separates conceptual problems from empirical ones, but this should not be taken as a separation of autonomous theoretical issues from non-theoretical questions of empirical validity. Although Laudan discusses particular conceptual issues such as inter- and intra-theoretical consistency (1977, Chpt.2), he also suggests that empirical problems are necessarily conceptually formed. As he states, empirical problems are not 'directly given by the world as veridical bits of unambiguous data' (Laudan, 1977: 15). Rather they are seen by us through our conceptual

⁴ In the second edition of *The Structure of Scientific Revolutions* Kuhn attempts to clarify precisely what the term 'paradigm' means, distinguishing two main uses that were prevalent in the first edition. In its more restricted use, 'paradigm' refers to an exemplary problem solution that is used by scientists as a model for developing solutions to current outstanding problems (Kuhn, 1970: 187-191). In its more general use, 'paradigm' encompasses a range of important cognitive resources that are shared by a community of practitioners, including symbolic generalisations, metaphysical beliefs, cognitive values, and shared exemplary problem-solutions (Kuhn, 1970: 181-7). For the purposes of this thesis I shall use the term 'paradigm' to refer to a disciplinary matrix. Models for problem-solving will be referred to not as 'paradigms' but as exemplary problem-solutions.

networks which preconstruct our understanding of the data, as well as providing expectations from which problematic phenomena deviate.

In each case, we find the authors defending the notion that scientific investigation is necessarily work within the theoretical realm. They argue that there is no direct connection of theories with the material world, but rather an engagement of scientists with the world mediated by theories. It is essential to stress, however, that the relative autonomy of theoretical science does not in any way 'protect' scientists from the problems and difficulties of their understandings. For all three authors considered above, the paradigms, research programmes or theories guiding research are inherently problematic in their application.

Looking again to Kuhn's work, we find that he discusses a range of more or less serious problems facing paradigms. The least serious problems are those that can be dealt with quite comfortably in the framework of the paradigm, and include things like increasing the scope or accuracy of the knowledge of important facts, increasing the match between a paradigm's predictions and the experimentally generated facts, and extending a paradigm into related areas of research (Kuhn, 1970: 24-9). This involves resolving problems which are of limited scope, and which practitioners are confident can be dealt with. Nevertheless, it is important to note that until a resolution is achieved, these are areas which a paradigm does not cover in its understandings, and which require theoretical development in order to be coherently grasped.

There are also problems of a potentially more serious nature. These are anomalies generated in the course of routine problem resolution which violate the 'expectations that govern normal science' (Kuhn, 1970: 52-3). Kuhn states that scientific research frequently uncovers new and unsuspected phenomena and these provide a challenge to the existing paradigm which will require its adjustment or replacement in order to turn an anomaly into 'the expected'. To Kuhn's mind, the awareness that an anomaly exists will not necessarily cause the abandonment of any particular paradigm (1970: 81). It's essential to note, though, that this is *not* because the mediated nature of theories protects practitioners from the problems with their theoretical practice. Rather, this is because all paradigms generate anomalies and discrepancies, and thus theory choices must be based on a sophisticated understanding of the advantages and disadvantages of competitors. What is important here is that Kuhn does not see paradigms as self-validating, and able to remove problems at will, but as always requiring development or replacement in order to cope with persistent difficulties in their application.

This same emphasis on the problems generated by the application of scientific theories can also be found in the work of Lakatos and Laudan. We saw above that Lakatos emphasised the 'protection' of certain theoretical elements from refutation in order to preserve the theoretical developments gained so far. This does not, however, dispel the problems of understanding generated by the theory. Protecting the 'hard core' does not allow the theorist to be rid of the difficulties with her or his understandings, and as Lakatos states, 'inconsistencies (including anomalies) *must* be seen as problems' (1978: 58). Rather, the defence of the hard core is a device to protect a certain set of insights and develop an elaborated theory around them that can make progress in resolving problems. Although theoretical development is often made, the existence of problems and anomalies for theories is a permanent feature of investigation. Lakatos suggests that even as successful a research programme as Newton's was "submerged in an ocean of 'anomalies'" when it was conceived, and no matter how long theoretical development continues, Lakatos argues that 'anomalies are never completely exhausted' (1978: 48, 49). The recalcitrance of the world presents a constant challenge to theories and for theorists.

Given Laudan's primary focus on problem-solving in science, it will not require a lengthy argument to demonstrate that he does not see theories as untroubled by problems and difficulties. Suffice to say, he argues that one of the primary activities of science is converting the apparent empirical anomalies of a theory into solved problems. Such activity recovers the initial meaning of the idea that 'it is the exception that proves the rule', in that the test of a rule is its ability to deal with what appear to be exceptions (Laudan, 1977: 31). Likewise, scientific activity involves attempting to remove conceptual problems such as those that occur when well-grounded theories contradict one another.

The moral that we can take from this line of philosophy is that the 'theoretical' character of scientific understandings does not result in the unproblematic 'construction' of the world on the scientists' own terms. Instead, problems and anomalies emerge from the application of theories, and these require theoretical labour and development to be removed. It is important to emphasise this, as some relativists have argued that because scientific knowledge is theoretical and mediated, problems of understanding can always be avoided. Particularly relevant here are the founders of the Strong Programme, Barnes and Bloor, who claim that a theory can always be construed as adequate to the phenomena that it deals with. As Barnes argues:

Conceptual fabrics can always be maintained by Duhem-type strategies so that, whatever their form, they remain both internally consistent and also consistent with experience...(Barnes, 1982: 106)

This would make theories entirely consistent on their own terms, generating none of the anomalies and problems which necessitate theoretical reconstruction. I shall analyse the Strong Programme argument in Chapter 4, but just indicate for the moment that post-positivist philosophy of science does not license this move, instead suggesting that the recalcitrance of the material world results in problems of understanding. As we have seen above, three of the major figures within post-positivist philosophy of science argue that even our very best scientific theories do not manage to avoid anomalies and problems. In their view, even these theories are not adequate on their own terms. The only point at which a theory could be said to be adequate on its own terms would be when it had such incredible grasp and capacity that no practitioners could identify any difficulties or anomalies for the theory. There is certainly no sign of such a theory emerging in any of the sciences, and its achievement would require a Herculean investigative and conceptual effort, not a philosophical argument about the nature of theories.

In opposition to a relativist account, a critical historicist approach suggests that we analyse theories in terms of their *conceptual adequacy*, that is, the variable coherence they achieve when accounting for the events of some domain. I will be discussing the notion of theoretical success in more detail when we turn to look at epistemological matters in science. However, on a first approximation, we can consider success to be a matter of ‘coping’ with the subject matter of a theory, or ‘making sense’ of it. If failure occurs when a theory generates anomalies and inconsistencies, success is produced by offering a coherent and systematic grasp of the outcomes of interactions with the world. Success involves using a theoretical framework to ‘resolve ambiguity, to reduce irregularity to uniformity, to show that what happens is somehow intelligible and predictable’ (Laudan, 1977: 13).

Now, this is clearly a very general definition, and may seem entirely unhelpful because of its vagueness. As I shall argue later on, there is a good reason for this, as ‘success’ denotes the ability of modes of reasoning to live up to their own standards. Nevertheless, this idea of success does offer a meaningful correlate to that of failure, in that it suggests that success involves gaining a consistent and systematic understanding of the processes or objects in question, such that when a theory is applied it can account for the behaviour of these. When a theory is successful, we find that in applying it to explain the subject matter of its domain, we produce experimental or observational outcomes that conform to our expectations. These successes are the ‘positive’ categories of the theoretical scheme. When a theory is problematic or inconsistent the result is that we cannot ‘make sense’ of outcomes, that certain results of experiment and observation are unexpected, and we cannot explain why

they occur as such. Such unexplained outcomes are the 'residual' categories of a scheme. To illustrate this with reference to Proutian chemistry, we might say that until the theory of isotopes was developed, those attempting to make sense of the atomic weights of elements could not account systematically for the atomic weight of chlorine, which was a residual category. It was only when the chemical theories were reconstructed to incorporate the theory of isotopes that sense could be made of this element and it could be accounted for within the positive categories of the Proutian theoretical scheme.

This conception of scientific knowledge thus locates both the successes and failures of a theory *within* understanding (Holmwood, 1996: 106-9). This suggests a way of avoiding two kinds of errors. Firstly, although a problem may be solved within a scheme, this does not validate the truth or correctness of that scheme for all time. The positive categories of a scheme encapsulate the best way presently known to account for some phenomena. However, we should not assume that the achievement of some coherency implies the truth of the theoretical claims, as categories may need to be reconstructed in order to achieve greater coherence⁵. To illustrate this, Laudan gives the example of Ptolemy's theory of epicycles, which resolved some of the difficulties in explaining the motion of planets, and thus achieved greater coherence over earlier accounts, but which belongs to a theoretical scheme now superseded by a more successful, coherent one (1977: 24-5). In another kind of case, what is considered an adequate solution in relation to the understandings of one particular generation may be rejected by succeeding generations as standards of experimental accuracy and conceptual adequacy develop. This was apparent in the success of the models of both Newton and Daniell Bernoulli in accounting for the relation between pressure and volume in gases, only for these to need important modifications as further experimental data accumulated (Laudan, 1977: 25-6).

The other error to be avoided is the assumption that because a theory contains contradictory, residual categories, it must be rejected entirely. The difficulties with such a move are illustrated by developments within falsificationist philosophy of science. On Lakatos' account, naïve falsificationism takes a severe attitude to contradictions between theories and accepted empirical claims, arguing that if contradictions exist, the theory must be removed from scientific contention (Lakatos, 1978: 24-7). However, as Lakatos notes, important theories have been considered scientifically acceptable even though they were in contradiction with some accepted empirical claims (Lakatos, 1978: 39). This was true, for example, of Einstein's theory when it was taken to be superior to that of Newton. Such

⁵ See the next section for further discussion of this point.

acceptance did not display the irrationality of the scientists involved, but suggests another policy in relation to contradictory theories. The existence of a contradiction is taken to indicate the need for further theoretical development, rather than the necessity to abandon a theory altogether. Taking this into account, the sophisticated falsificationist argues that the key to scientific development is not the complete rejection of contradictory (falsified) theories but the attempt to develop a theory by removing its contradictions in a productive manner (Lakatos, 1978: 40-1). Holmwood concurs with this point, adding that even a contradictory theory usually has some positive categories which offer a degree of resourcefulness (Holmwood, 1996: 106-7). Where the theory is systematic and consistent it allows us to account successfully for the world; where the theory is contradictory and inconsistent, it generates confusion and unpredictable results. Until problems are resolved, the use of an existing theory is the only option available to scientists.

The account so far has emphasised the importance of producing conceptual consistency. The material pay-off for this, however, is an increase in the capacity for action. In general, the more consistency that a scientist can generate in their accounts of the behaviour of processes and objects, the greater their ability to deal with this domain successfully in the future⁶. Within a scientific context, this may mean successfully producing desired experimental outcomes, or contributing to the development of technologies for harnessing the powers of the objects or processes in a domain. Interactions with objects that had previously led to unpredictable and unintelligible outcomes become readily comprehensible due to the increased ability to offer coherent accounts. It is this increased practical capacity which provides the tangible material benefit of a successful conceptual understanding.

The practical impact of problem-solving upon action emphasises its *necessity*. This is in contrast to relativist accounts, which suggest that problems with theoretical schemes can always be removed in order to make those schemes completely consistent. If this was the case, interactions with the world could invariably be shown to be successful by using 'Duhem-type strategies', and so there would never be any necessity to alter a theory in order to improve its capacity for prediction and control. Events in the world would never pose any difficulty for actors in their attempts to achieve their goals⁷. In opposition to this, the

⁶ Of course, conceptual resourcefulness may not always have a pay-off in practical resourcefulness. Technical difficulties of collecting the relevant information before events occur may mean that even though the processes at work are understood by existing theories, this knowledge does not translate into increased practical capacity.

⁷ As we shall see in Chapter 4, Barnes and Bloor attempt to defend both relativism and instrumentalism. Ultimately, the relativism wins out, and they argue that variable success is not an issue for theories.

practical account offered here suggests that problems of understanding are ‘lived’ by actors, who experience them as an incapacity to act successfully, and achieve the outcomes that they desire. The inability of actors to deal successfully with the world generates a tension which motivates problem-solving activity oriented towards an improvement of capacity.

Of course, not all kinds of conceptual sense-making necessarily contribute to action, and so ‘practical adequacy’ is a subcategory of ‘conceptual adequacy’. One particularly important variety of sense-making which may not have any practical import is that of historical understanding. For instance, we may ask whether Rasputin really did survive a dose of powerful poison and three gunshot wounds only to drown in the Malaia Moika canal into which his body was disposed⁸. One could of course argue that all historical knowledge might have implications for future human activities. This is a possibility, although given the sometimes minute details unearthed by historians, the probability of practical relevance may be incredibly low. Nevertheless, these activities involve making consistency and regularity out of disorder in order to understand events that have occurred. They can thus be assessed under the rubric of conceptual adequacy.

1.4 Realist alternatives

In the previous sections I developed the critical historicist idea that scientific knowledge is a form of sense-making activity which generates the ability for humans to interact successfully with the environment. However, not all philosophers of science would be happy with this characterisation, and there are two important positions in the field which offer quite different views of the nature of scientific knowledge. These are epistemological realism⁹ and ontological realism. For epistemological realists, an emphasis on the successful sense-making activities of science provides an insufficient account of the nature of knowledge. Epistemological realists argue that when a theory is successful, this gives us grounds for thinking that it corresponds (to some extent) with the world. In other words successful theories have some degree of truth. By contrast, ontological realists reject the inference to the truth of our theories from their success. However, they wish to make a strong separation between scientific understandings of the ‘deep structures’ of the world and the successful accounting for patterns of events. For ontological realists, genuine scientific

⁸ Richard Pipes suggests that this was not the case, as the autopsy revealed Rasputin to have been dead before he was thrown into the canal, and there is strong evidence that there were no traces of poison in Rasputin’s body (Pipes, 1990: 258-66). This raises new and interesting problems of consistency, however, given that accounts of events leading up to Rasputin’s death suggest that he had consumed wine and pastries laced with potassium cyanide.

⁹ Also known as ‘scientific realism’ (see for example Psillos, 1996).

knowledge is oriented to the structures of the world, and may not result in successful accounts of the events which actually occur. I shall address epistemological and ontological realism in turn, arguing that both have a conservative tendency to reify current, successful scientific knowledge, implying its adequacy (either approximation to truth or explanatory correctness) in the face of problems of understanding.

Recent debates over epistemological realism (henceforth E-realism) were fuelled by Putnam and Boyd, among others, in the 1960s and 70s. Putnam suggests that the clearest cases for inferring truth from success are the 'mature' sciences (such as physics) which have a long and venerable history (Putnam, 1978: 21). In the mature sciences, we can assume that the theoretical laws are 'typically approximately *true*' because there is no other reasonable explanation for their long-term success. To take one of Putnam's examples, if physics is wrong about the metric structure of space-time then 'it is a *miracle* that a theory which speaks of curved space-time successfully predicts phenomena' (Putnam, 1978: 19). Leaving aside the miracle 'explanation' as a last resort, E-realists believe the only plausible alternative to be that in the course of long-term successful development scientific knowledge converges upon the truth about the world. It is no miracle that physics is successful, because it has discovered the true structure of the world, and captures this in its theories. Epistemological realists thus use a form of abductive argument, or inference to the best explanation. The best explanation for success is some form of correspondence with the world.

Of course, not all sciences are mature, and some sciences that we might now call mature have not always been so. Nevertheless, these sciences often have a certain degree of success, and this also needs to be explained by the E-realist. In such cases, these theorists employ the notion that a successful theory is 'partially true', and argue that succession in scientific theories involves a 'partially correct/partially incorrect account of a theoretical object' being 'replaced by a better account of the same object or objects' (Putnam, 1978: 19). So, by E-realist standards, even limited theoretical success is to be understood as the accurate correspondence of a theory with some aspects of the world.

This connection between success and accurate correspondence is not only epistemological. For E-realists, it also has important ramifications for our attitude to theories. Firstly, if a field is mature, then laws which are intended to succeed those already postulated (and so approximately true) must retain several of their features. The new laws must not only conserve the true predictions of the old theory, but also retain the earlier theory as a "limiting case" (Putnam, 1978: 21), that is, demonstrate the correctness of the

values assigned to theoretical variables in the earlier theory within certain boundary conditions (which are clearly to be more limited than those in which the later theory operates). This is not to be viewed only as an instrumental connection between the two theories but one which implies the '*approximate truth of the theoretical laws of the earlier theories in certain circumstances*' (Putnam, 1978: 20. Author's emphasis).

There are two further requirements derivable from E-realism, these applying to immature science. Scientists should attempt to conserve the mechanisms of earlier theories 'as often as possible', and be able to assign meaning to the referents of the old theories in the new theories (Putnam, 1978: 20, 22). This conservation is to be encouraged because of the connection between success and (partial) truth. Given the E-realist view that the only viable explanation for success is partial truth, any time that a theory has been successful scientists are obliged to retain at least some aspects of it - those which contributed to its success and therefore partially correspond to the nature of the world. Citing Boyd's work, Putnam suggests that scientists typically do follow these strictures, preserving the mechanisms of earlier theories or showing them to be limiting cases of new mechanisms (Putnam, 1978: 20). If scientists actually do follow these policies, E-realism gives an account of the good reasons why they do so.

The classic response to these E-realist views is Laudan's piece 'A Confutation of Convergent Realism' (1981a), the substance of which also appears in his *Science and Values* (1984). Although his discussion ranges across a number of issues, the central target for Laudan is the claim that success and truth are closely connected in that the truth of a theory (its accuracy in corresponding to the world) provides an explanation for its success. Laudan disputes this claim in several ways.

One fundamental problem with the E-realist account, on Laudan's view, is the idea that successful theories within science can be seen to be 'approximately true'. Laudan sets up his argument by suggesting that theories can only claim approximate truth if their central (success-generating) theoretical terms are held to refer (i.e. their postulated entities actually exist). However, the history of science contains a large number of successful theories whose main theoretical entities are not, by the standards of current science, held to exist. These include: the humoral theory of medicine; the effluvial theory of static electricity; the caloric and vibratory theories of heat; the vital force theories of physiology; aetherial theories; and theories of spontaneous generation (Laudan, 1984: 113, 121). Because we do not believe that the entities and mechanisms postulated by such theories exist, the success of

the theories cannot be explained by their approximate truth in corresponding to those aspects.

Laudan considers a possible response to this argument. The epistemological realist could claim that the only valid grounds for claiming 'approximate truth' are that a science is mature, and that this might eliminate many of Laudan's examples (1984: 116). However, suggests Laudan, this is unconvincing for two reasons. Firstly, the definition of what constitutes a 'mature' science by epistemological realists is very loose, and without rigorous criteria for defining this stage, a lack of 'maturity' can be claimed any time a theory turns out not to be (by our standards) approximately true. For E-realists, though, maturity is supposed to be an independently specifiable criterion which, as we saw above, actually sets restrictions on the kind of theory change that can legitimately occur. Without properly formulating a definition of mature science, epistemological realists can always avoid confronting cases which appear to challenge their account.

Secondly, E-realists are not only committed to the approximate truth of mature science but the partial truth of any successful science. This is an aspect of their inference to the best explanation, because if some successes can be explained without using the notion of adequate correspondence, this undermines the argument that correspondence is the only reasonable explanation for the success of science (Laudan, 1984: 22). The cases put forward by Laudan then provide a difficulty for this position. Although these theories had genuine success, the fact that we no longer accept that their entities or mechanisms exist suggests that we cannot describe the theories as even 'partially true'. We should note that an instrumentalist explication of this concept is too weak to fill this requirement, as E-realist theories are designed to push the notion of truth beyond instrumental success and connect it with the really existing entities in the world. Accounting for the success of non-referring theories thus becomes a problem for E-realism, and a challenge to its inference.

Aside from these epistemological concerns, Laudan also questions the E-realist emphasis upon the retention of successful theories. On the epistemological realist view, if a theory is successful, and thus partially true, then scientists should only pursue later theories that 'retain the appropriate portions of earlier theories' (Laudan, 1984: 124). As we have seen, in the case of mature sciences the burden of past success is particularly strong, leading to strict requirements to retain the entities and mechanisms which have produced that success.

Laudan's response to the argument for retention is to suggest that there are frequent cases in the history of science where the key mechanisms of an earlier successful theory are not retained by the later theory as a limiting case. The examples that he cites include the shift

from Ptolemaic to Copernican astronomy, the move from classical to relativistic physics (which did not retain the mechanisms of the aether), the move from Darwinian to modern genetics (which did not retain Darwin's notion of pangenesis), and the move from the corpuscular to the wave theory of light¹⁰ (Laudan, 1984: 127). Of course, we might view these cases as aberrations on the part of the scientists involved, and see these as problematic, irrational episodes in science. Given our positive evaluation of the successor theories, this seems unlikely, and Laudan suggests that

some of the most important theoretical innovations have been due to a willingness of scientists to violate the cumulationist or retentionist constraints which realists enjoin mature scientists to follow. (Laudan, 1984: 127)

It seems, then, that epistemological realism offers poor advice to practising scientists¹¹. One perfectly legitimate way for science to move forward is for a theory to be replaced by one which is more successful, but which does not retain a commitment to the entities and mechanisms of the earlier case¹². As Laudan (1984: 130-1) points out, epistemological realism is 'reactionary' in opposing such change, and forecloses the prospect of deep ontological shifts in our existing theories (particularly in the mature sciences). Contrary to E-realism, it is possible that future scientists will conclude that some or most of our successful theories in fact do not capture the 'truth' about the world, but postulate entities which come to be viewed as non-existent, in the same way as 'phlogiston', 'aether' and 'caloric' (Laudan, 1984: 131). The inference of approximation to truth as the 'best explanation' of success comes to be a barrier to successful theoretical development,

¹⁰ Of course, listing such cases is merely indicative, and a proper evaluation of these issues requires close attention to these examples. Laudan discusses further cases elsewhere (Laudan, 1976), one example of which is the caloric and early kinetic theories of heat. He argues that one of the main explanatory successes of these theories was their ability to explain the generation of heat by friction. However, due to problems dealing with other phenomena, the kinetic theories of heat were rejected in favour of substantial ones, despite the fact that the latter could not explain frictional heat. Here we have a theory change in which the loss of a successful mechanism did not cause the successor theory to be rejected.

¹¹ Furthermore, it is not at all clear that scientists do actually follow realist principles in their own theory assessments. Obviously this is a sizeable empirical question, but Laudan cites a number of examples which at the very least cast doubt on this notion. He argues that in a range of important theory shifts, from catastrophist to uniformitarian geology, from Lamarckian to Darwinian evolutionism and from the corpuscular to the wave theory of light, none of the later theories were criticised by scientists at the time for failing to preserve the theoretical mechanisms of the earlier, successful theories (Laudan, 1984: 126).

¹² Putnam misleadingly suggests that the desire of scientists to preserve theories in the face of anomalies is good evidence of a retentionist strategy (Putnam, 1978: 20). However, it seems equally likely that scientists are not preserving the specific mechanisms and entities of a theory (on the grounds that they are somewhat truthlike), but are conserving success until a theory comes along that can demonstrate its pragmatic superiority. As we saw above, given that all theories retain problems and anomalies, these do not provide grounds for theoretical rejection on their own.

recommending conservative theory choices over those which other sense-making criteria support.

Of course, the debate over epistemological realism did not come to an end with Laudan's contribution, and E-realists such as Leplin (1997), Kitcher (1993), and Psillos (1996) have developed responses to Laudan's claims. Although their arguments are somewhat diverse, the main E-realist tactic is to distil a notion of 'success' that is tight enough to warrant an inference to the truth of a theory characterised as successful in that way. Leplin (1997) does this by arguing that only a certain subset of theories, those which have produced novel predictions, are best explained by their partial truth. Psillos and Kitcher take a somewhat different approach, attempting to trace back through scientific developments to discover which parts of earlier theories contributed to their success and which did not. On these grounds they hope to show that the parts of theories which contributed to success were those retained by later theories. If this can be done, then one cannot claim, as does Laudan, that successful theories of the past have been falsified by new developments; genuine success can be reconnected to truthlikeness.

Interesting though these arguments are, they fail to deal with the strongest argument against epistemological realism. No matter how carefully demarcated the list of theories argued to be true/partially true is, the claim for their truthfulness is being made in a situation where science is incomplete, and some phenomena are as yet unexplained. In such a situation, there is no guarantee that even our best, most successful scientific theories will not have to be reconstructed in order to account for those unaccounted occurrences. In other words, no matter how good the positive categories of a theory are, they may need to be reconstructed in order to account for residual phenomena. The attempt to designate theories (or parts of theories) as true operates to reify those categories that are currently successful, even though scientists' attempts to continue improving knowledge may require the reconstruction of those theories. In many respects this reification is a more ambitious one than that employed in defence of an unquestionable observation language, as the E-realist argument for the truth of theories must rest on the indubitability not only of simple observational predicates, but large chunks of theoretical argument.

Of course, if the success achieved by a superseded theory is not a product of chance, we would expect that the regularity it accounted for will be made explicable by a later theory¹³.

¹³ Laudan argues that this should not be taken as a necessary criterion for the greater adequacy of a later theory, as there may be other grounds for preferring it (Laudan, 1977). However, we would expect that the regularities discovered by an earlier theory will come to be explicable as our knowledge develops.

It is important to see, however, that this is not the same as a theory being 'partially true', in the sense referred to by epistemological realists. In E-realist terms, partial truth means the adequate correspondence of some part of the theory to the world as it actually is, with the consequence that some categories must remain unchanged because of their truthfulness. In the analysis offered here, if a theory is 'partially adequate', it usefully grasps a regularity which should be accounted for by later theories, although those later theories need not use any of the terms of the earlier accounts.

I would argue that this undermines the force of the E-realist 'no miracle' argument, which articulates the intuition that the success of a theory must be a mystery if it is not at least partially 'true'. The critical historicist response is that we expect the regularities discovered by earlier theories to be explicable, but they may well be part of a wider pattern which can only be accounted for in terms not reducible to those of the earlier theory. Once again, we can think of the shift from Newtonian to Einsteinian theories of mass; Einstein's theory makes sense of a wide range of regularities, including those accounted for by Newton's, but does not do so using Newton's terms. E-realists imply that the success of an earlier theory must be *either* 'miraculous' *or* the result of that theory's 'partial truth'. Instead, we can argue that (a) a theory was successful because it accounted for some regular fragment of a wider pattern of phenomena (thus its success was not a miracle); and (b) a change of theoretical terms was required to account for the wider pattern (thus the earlier theory was not partially true). Successful theories from earlier scientific activity need not be thought of as partially true, but simply partially adequate (as are all theories until the completion of science¹⁴).

If epistemological realists reject the sufficiency of conceptual adequacy on the grounds that the required inference to truth is not made, ontological realists reject it on the grounds that science is not oriented to the pattern of events that occur, but understanding the structures generating those events. The key writer here is Roy Bhaskar, whose works have been extremely influential within British sociology, particularly in its Marxist forms. I shall be arguing that although his claims have different grounds than those of E-realists, they ultimately produce the same effect by encouraging the hypostatisation of current successful science.

¹⁴ It is important to note the extent to which this licenses 'pessimism' about science. Certainly, E-realists are correct to see that pragmatist arguments encourage pessimism about the possibility that our current theories are 'true' or 'approximately true'. However, if one takes the measure of science to be pragmatic adequacy, then there is no cause for concern, given that later (untrue) theories are often more adequate than earlier (untrue) theories.

For Bhaskar, it is crucial to distinguish two domains of reality, the real and the actual, between which there is an ontological gap¹⁵ (Bhaskar, 1975). The domain of the real is the deeper level, and consists in the basic mechanisms which generate surface events at the level of the actual. One way to illustrate this split is to use the example of a clock: inside a clock there is a mechanism (the domain of the real) which generates the movement of the hands on the surface (the domain of the actual). However, the parallel between a clock and the complexities of event generation in the universe is limited, and this difference illustrates Bhaskar's key argument. In a clock there is a single generative mechanism which (except in rare cases of mechanical failure) is constantly conjoined with the position of the hands on the clock face. In the universe, conversely, there are many complex and heterogeneous mechanisms generating the surface outcomes at the level of events. It is as if underneath the clock face, there were hundreds or thousands of mechanisms all pushing and pulling the hands in different directions, thus generating a complex and chaotic set of hand-positions on the face itself.

The separation of the 'real' and the 'actual' necessitates a parallel distinction, between explanatory and predictive knowledge. For Bhaskar, explanation is an activity concerned with locating and theorising the deep structures that generate outcomes on the surface of events, whereas prediction is oriented only to the actual events themselves. There are, according to Bhaskar, some occasions when the two activities coincide. Such occasions arise in 'closed systems', that is situations where only one generative mechanism is able to express itself, the others being held constant or removed from influence. When there is a closed system, explanations and predictions line up with one another, and if we can explain the character of the mechanism at work, we can predict what the outcome will be. However, closed systems rarely emerge spontaneously, and are typically brought about by human experimental activity. The vast majority of situations involve open systems, characterised by the chaotic play of complex and qualitatively different processes and objects.

When dealing with open systems, the tasks of explanation and prediction are quite different. Explanation in open systems is concerned with locating the antecedent events and underlying tendencies or processes that contributed to the events that actually occurred. It thus deals with the level of the real. Prediction, however, is directed only towards the events themselves, that is, the actual outcomes. Because of the chaotic nature of open systems, although our predictions may be defeated, no explanatory theory is 'disconfirmed by the

¹⁵ In fact, Bhaskar separates off a third level termed the 'empirical' which is the level of experienced events (Bhaskar, 1975: 56). However, this distinction is not important to my discussion here.

contrary behaviour of the uncontrolled world' (Bhaskar, 1975: 119). We may have a justifiable explanatory understanding of some mechanism(s) but find that in an open system other mechanisms are influencing events, leading to unpredictable results.

According to Bhaskar, the difference between explanation and prediction is highlighted in certain scientific areas where they are clearly distinct. For example, Bhaskar suggests that in the field of meteorology the predictions that can be offered are fairly unsound because of the 'instability of the phenomena' (Bhaskar, 1975: 119). Nevertheless, good explanations can be offered after the event, drawing on physical laws that have been confirmed by experiments using closed systems¹⁶. Likewise, good explanations may be less useful for prediction than simple generalisations or rules of thumb, because of the difference between studying mechanisms and studying events (Bhaskar, 1975: 136). The difference between explanation and prediction can also be emphasised by pointing to the distinct skills required for each. Good prediction is holistic, involves thinking at different levels at once, and putting together diverse bits of information, whereas good explanation requires individuation, focusing only on the specific mechanism of interest, and filtering out information about all others (1978: 120).

Having so far discussed Bhaskar's separation of explanation and prediction, it is important to indicate that he does not view the two activities as equally central to science. Rather, the purpose of science is to gain explanatory knowledge of the real mechanisms that operate in the universe. As Bhaskar puts it:

the ultimate objects of scientific understandings are neither patterns of events nor models but the things that produce and the mechanisms that generate the flux of the phenomena of the world. (Bhaskar, 1975: 66)

Science is concerned with an *explanation* of the level of the *real*. Successfully accounting for the pattern of events, of which prediction is one aspect¹⁷, is neither a primary concern nor a necessary result of scientific investigation.

¹⁶ Bhaskar's position is that explanation in open systems must rely on laws confirmed in closed systems. He states in his critique of falsificationism that 'events may be explained in open and closed systems alike, but...law-like statements may only be falsified under effectively closed conditions (where deductive test predictions are possible)' (Bhaskar, 1975: 136). As Collier (1994) notes, however, this raises problems when it comes to explanations in social science. This is because Bhaskar insists that societies are open systems, leaving no possibility of generating social scientific laws through 'deductive test predictions'. Although Bhaskar makes some remarks in *The Possibility of Naturalism* about possible experiment analogues, these seem weak, and Bhaskar does not fully face up to the problem of explanation in social science.

¹⁷ As noted earlier, accounting for events of the past properly may not have a predictive pay-off, making prediction a sub-category of wider sense-making activities.

On the face of it, these arguments undermine the idea introduced earlier that the conceptual adequacy of a theory is to be measured by its ability to offer a coherent account of events. If they are correct, we must distinguish knowledge of the objects and processes of a domain from knowledge of events, prioritising the former. However, we can start to probe Bhaskar's arguments by examining the gap between the real and the actual. Bhaskar is certainly correct to emphasise that outcomes at the level of events are typically generated by a large number of influences. By itself, this does not necessitate the inference that knowledge of the real will necessarily be distinct from knowledge of the actual. In fact, on this model, we can argue that in circumstances of complete knowledge, our account of the real (i.e. postulated structures) and the actual (i.e. events that occur) should converge. As explanatory knowledge improves, we understand more of the structures generating the actual, and thus improve our understanding of the pattern of events. A full explanatory understanding of the real would result in a completely coherent account of occurrences on the level of the actual. On this view, any difference or gap between our account of the real mechanisms and occurrences on the level of the actual is a *problem* of understanding. Explanatory understanding in such cases is a limited form of the knowledge of events, not a superior mode. Defending explanatory categories where they do not account for the outcomes they are held to generate involves a refusal to reconstruct one's positive categories in the face of explanatory problems.

On this basis, we can question whether Bhaskar's use of meteorology really backs up his argument about the split between explanation and prediction. His claim is that although meteorologists' predictions frequently fail, they use well-grounded explanatory laws to 'retrodict the antecedent events and states by means of which they both explain what actually happened and excuse their forecasts of it' (Bhaskar, 1975: 119). However, we can legitimately ask why laws which provide a good post hoc explanation cannot be used to provide an accurate prediction. Presumably, the reason for this is that information contingently lacking beforehand is available after the event, so that scientists can then propose an adequate explanation. The difference between prediction and explanation in this case is thus no more than the difference between an account of events offered without all the required information (prediction), and an account of events offered with all the required information (explanation). There is no reason to postulate an ontological distinction to account for this difference.

Apart from arguments which rest upon contingent issues of information, Bhaskar offers other reasons why explanation and prediction, and thus knowledge of the real and the actual,

must be viewed as distinct. These reasons are based upon there being an ultimate lack of fit between the real properties of structures and things, and the events that occur on the level of the actual. For this to be the case, it is not enough that the actual does not clearly display the effects of one structure because of the multiple determinations operating. Rather, it must be that even if we were to fully understand all structures, mechanisms and properties at the level of the real, we would still be unable to account for all events on the level of the actual. As Bhaskar puts it:

there is a distance between the laws of science and the ordinary phenomena of the world, including the phenomena of our actual and possible experience. (Bhaskar, 1975: 111)

Bhaskar puts forward two main types of case in which the actual events are not determined by real structures. The first of these is the operation of agency, the second the presence of heterogeneity and multiplicity of influence, and although they overlap to some extent, I will treat them separately here.

Bhaskar's argument about agents is that there are items in the world whose behaviour cannot be understood as a 'determined' response to some set of antecedent conditions. The complexity of certain items means that no amount of knowledge of the causes operating upon them can lead to an accurate prediction of the outcome. Scientific laws do not determine the behaviour of such 'agents', but only set limits on their activities which act as a constraint of possibility (Bhaskar, 1975: 105). Bhaskar thus argues for the 'autonomy of things' (Bhaskar, 1975: 108), in that there exists agent-centred causation which is not the result of an antecedent condition (1975: 107). Agents are not fully determined by the laws of the universe, and even if we had complete knowledge of such laws, we would still be unable to account for the actual events that occur. Bhaskar states:

From the normic and non-empirical nature of laws...I conclude that the world is a world of agents incompletely described. (Bhaskar, 1975: 114)

Although Bhaskar's definition of 'agents' as 'particulars which are centres of power' (1975:111) is a very broad definition, one of his paradigm cases is that of human beings¹⁸. Human beings have to conform to physical laws in their activities but are not determined by them.

¹⁸ This is unsurprising given that Bhaskar's philosophy of social science relies on the idea that one can distinguish between 'structure' and 'agency'. In fact, it could be argued that Bhaskar's emphasis on the notion of agency in natural scientific phenomena is largely intended to set the scene for his social scientific theory.

As Bhaskar puts it:

What I can do is constrained by the operation of natural laws. But I can hack my way all over the physical world, defeating empirical generalizations. (Bhaskar, 1975: 113)

For Bhaskar, then, agency operates in situations where the behaviour of items cannot be accounted for solely by antecedent conditions (Bhaskar, 1975: 107).

The central case where 'agency' applies is that of emergence, where a new level of being is generated whose behaviour cannot be explained by the laws of a 'lower' level. This process is well-exemplified by the emergence of organic life from physical or chemical elements, generating a new set of materials which have causal powers not reducible to those operating on the physio-chemical level (Bhaskar, 1975: 113). Although organic life is bound by those lower-level laws, it nevertheless has the ability to 'determine the conditions under which physical laws apply' (Bhaskar, 1975: 113). For example, although human beings are constrained by physical laws, they can nevertheless engage in a wide variety of activities, the choice of which is not determined by those laws. The same partial autonomy from lower levels is manifest in all cases of emergence, the higher level structures having properties irreducible to those of the lower level.

Agency is thus associated with emergence. But it is questionable whether this establishes the 'autonomy of things' referred to by Bhaskar. He seems to be arguing that agency involves a certain degree of 'freedom' or even 'self-determination' within the parameters of the real structural forces operating in the universe. It is on this basis that the real and the actual might not line up with one another, our knowledge of the real not encompassing agentic activities which would produce unpredictable outcomes on the level of the actual. Thus, Bhaskar remarks:

There is a space between the laws of physics and physiology and what I do within which deliberation, choice and voluntary behaviour have room to apply. (Bhaskar, 1975: 111)

However, as Collier (1994: 129-130) notes, Bhaskar ends up arguing only that some levels of activity are relatively autonomous from other levels. This does not establish that occurrences at higher levels are not determined by real structural forces, just that they are not determined only by the real structural forces operating at a lower level.

As Bhaskar concedes:

...the behaviour of e.g. animate things is not determined by physical laws alone. But that does not mean that their behaviour is not completely determined: only that an area of autonomy is marked out which is the site of a putatively independent science. (Bhaskar, 1975: 114)

Presuming, as seems reasonable, that the new science involves discovering the real structural determinations operating at a higher level, it is not clear that we must invoke 'agent' causality as well as 'antecedent' causality. It may well be the case that human activities cannot be explained using physical laws alone. This does not entail that they are inexplicable using any laws, merely that we need to add in our understanding of various biological, psychological and socio-cultural structures to provide a full explanation. What is required is a careful appraisal of the different causes operating at different levels and their interplay. As our knowledge of 'real' determinations progresses, we should gain an increasing ability to account for occurrences on the level of the actual. There is thus no reason why the emergence of relatively autonomous levels of being should result in a misalignment of the real and the actual, as Bhaskar implies.

The idea that emergence generates unpredictable agency is a sub-category of Bhaskar's more general argument that the multiplicity of determinations operating in the universe generates a split between the real and the actual. He suggests that:

...it is possible to give a complete explanation of an event without thereby being in a position to deduce it, namely if the different generative mechanisms at work are of radically different kinds... (Bhaskar, 1975: 137)

A complete explanation may fail to account for the actual events if the mechanisms involved operate on different levels, because we may have no knowledge of the 'mode of articulation' of these levels (Bhaskar, 1975: 119). For Bhaskar, lack of such knowledge should not be taken to be a failing of science, but a consequence of the complexity and multiplicity of the world (Bhaskar, 1975: 125). As Bhaskar states:

The transcendental realist sees the various sciences as attempting to understand things and structures in themselves, at their own level of being, without making reference to the diverse conditions under which they exist and act, and as making causal claims which are specific to the events and individuals concerned. (Bhaskar, 1975: 78)

Our knowledge can thus only extend to understanding structures 'in themselves' and 'at their own level of being'. We may have full explanatory knowledge of structures but find ourselves unable to understand the outcome of their interplay.

This argument has not convinced all of those with realist sympathies. Ted Benton argues that the existence of complexity and plurality does not rule out predictive understanding of actual outcomes. After all, even if multiple mechanisms are at play, why shouldn't we be able to calculate their resultant effects? Predictions made on such grounds may not be perfect, and delimit a range of possibilities rather than a single outcome, but they could be reliable none the less (Benton, 1981: 18). As with the case of emergence, complexity does not necessarily lead to unintelligible outcomes on the level of events, but the need for understanding of the actual to take into account several levels of determination.

For Bhaskar to avoid this response, he needs to argue that the interplay of different mechanisms may generate event-level outcomes which cannot be explained by reference to the 'real' characteristics of the structures involved. Only in this way can he insist that one can have a full explanatory understanding of the real, and yet be unable to predict events at the level of the actual. However, such an argument starts to look like dubious theoretical protectionism in which the real structures proclaimed by the theorist to be generating events are defended even though they cannot account for the events that actually occur. In other words, the 'real structures' are revealed as the positive categories of the theorist's system which s/he refuses to reconstruct even when they cannot account for the phenomena in question, leaving some actual events unaccounted for (residual). The claim to be addressing theoretical structures 'in themselves' and 'at their own level of being' operates as an arbitrary restriction upon what we might come to know about the structures generating events in the world¹⁹. If there are surprising interactions between different kinds of structures, these must surely be explicable in terms of the real properties of those structures involved, and thus a subject for knowledgeable investigation. We cannot assert that our knowledge of the relevant mechanisms is explanatorily complete when these mechanisms combine to make new and interesting outcomes that we do not understand. Rather, we should view surprising outcomes as anomalies which we must account for by developing our understanding. A full explanatory understanding of the real structures generating events would thus 'line up' exactly with the pattern of events on the level of the actual. Neither emergence nor the complexity of determinations involved in generating events suggests that knowledge of the real and actual should be separated because of an ontological gap between

¹⁹ It is not clear what the phrase 'level of being' is supposed to delineate here. Presumably any time that a structure influences events that constitutes a relevant expression of its 'being'. One can only surmise that the 'level' Bhaskar is indicating is constituted by the properties of the structure already known. To reify these understandings on the basis of ontology seems dubious given the possible need to reconstruct concepts as 'structures' to better account for events.

their domains. As Holmwood and Stewart point out, the attempt to separate the characteristics of levels in this manner is a problematic response to explanatory failure. By arguing that the levels have separate characteristics, theorists avoid acknowledging that the lack of coherence between levels reveals problems of understanding to be resolved (Holmwood and Stewart, 1991, especially Chpt. 4). Bhaskar's arguments create a gap into which scientists can push their explanatory problems, helping them to avoid reconstructing their explanations when they fail to deal successfully with reality²⁰.

1.5 Assessing understanding: commensurability and progress

So far in this chapter I have been outlining a view of scientific practice as an ongoing sense-making activity which has no guarantee of success. I have defended this critical historicist approach against various forms of realism, claiming that by locating adequacy outside of understandings (in 'truth' or 'real structures') successful understandings are treated as if they need no further reconstruction. However, one crucial issue has yet to be broached. We have yet to consider the validity of scientific theories, and the warrant that can be claimed for them. Such matters are particularly pressing when we consider the frequency with which conflicting theories arise within science. Here I will be arguing that the notion of the most conceptually adequate theory can be developed to account for progress in understanding in a way that requires neither external reference nor fixed standards of knowledge in order to avoid relativism. Knowledge should be understood as a repository of the best conceptual achievements developed so far, that is the most successful modes of accounting for events in the world currently available. However, to establish this will require discussing and rejecting claims that such accounts must collapse into relativism.

It was argued at the beginning of this chapter that scientific activity has no guarantees. This claim involves two elements. Firstly, there are no 'facts' or 'structures' in the world that we can apprehend with such certainty that our understandings of them may not be reconstructed if necessary, in order to make better sense of our interactions with the world. Secondly, there are no standards governing the validity of our understandings that guarantee a successful interaction with the world. To some extent, we can see this as a good thing, suggesting as it does the continued room for human creativity and ingenuity in the process of making knowledge. Scientists are not mere automatons filling out routines that guarantee

²⁰ One might suggest that Bhaskar's adherence to Marxism involves treating the structural mechanisms proposed by Marx as real, and consigning the anomalies generated by them to agency and complexity. This avoids the task of reconstructing Marx's categories to turn these anomalies into elements that are systematically accounted for.

the validity of the understandings produced. More problematically, it means that the spectre of relativism lurks ominously over our claims to justification. Within the twentieth century, a large proportion of the anti-relativist thinking within the philosophy of science has attempted to employ stable empirical facts, referents or scientific standards in order to guard against relativism. Many thinkers would argue that if none of these conditions can be fulfilled, then relativism must ensue. Here, I will be attempting to provide plausible arguments that this is not the case. To do so, I shall be drawing upon ideas from the work of Laudan (1984, 1996) and Shapere (1984), both of whom offer historically sensitive accounts of the development of understanding in science, while trying to avoid the relativist conclusions of historically oriented thinkers such as Kuhn and social constructionists.

Issues of theory comparison and relativism have been tackled in a variety of ways within the philosophy of science. Here I want to argue that theory comparison is a test of the conceptual resourcefulness of theories. As such, I will focus on the logic of particular theoretical disagreements and argue that the goal in such cases is to discover if one theory is demonstrably more coherent and systematic than another. From this perspective, the most important question is: does one theory provide a more successful mode of accounting for the material world than its competitors? If we can reach reasoned decisions about the adequacy of theories, then this provides a basis for our theoretical judgements. Such assessments of validity can also be used to analyse whether a series of theoretical developments is progressive or otherwise, depending on its increasing or decreasing resourcefulness.

The relativistic alternative to this perspective suggests that we cannot distinguish between the validity of competing theories. However, this argument must be stronger than the claim that it is not always possible to distinguish which of two competing theories is more valid at some particular time and state of evidence. Those emphasising the rationality of theory choice would accept that there is sometimes not enough evidence yet gathered to settle competing claims. Rather, relativists must demonstrate that important theoretical disputes cannot be resolved *in principle*. If this can be demonstrated, then we must accept that competing theories are incommensurable, and that it is not possible to decide which accounts better for events in the world. I will be hoping to avoid this conclusion by indicating how theoretical disputes can, in general, be resolved.

In order to think about the development of understanding, it is useful to start from the most elementary form of theoretical comparison, and use this as a basis to work towards a more sophisticated notion. Such comparison is limited to assessing the adequacy of the substantive categories of a theory. In discussing this, we must temporarily set aside the fact

that theoretical standards may be different for competing theories, and examine how judgements of superiority can be made if we assume a stable set of scientific standards shared by competitors.

The most simple form of theoretical progress is that of a single theory developing over time. From the post-positivist perspective discussed earlier, this kind of development is achieved when a certain group of categories are persistently applied by a community of scientists, in order to expand their ability to account for the world. In this situation there are two types of element, the set of systematic understandings which orient scientific activity, providing some ordering of the domain, and the set of anomalies which cannot be systematically dealt with at the present time. The aim of scientists is to develop the existing understandings, or positive categories, in order to incorporate more of the phenomena that are currently anomalous, occupying residual categories. This is the kind of progress envisaged by post-positivist philosophers of science, for instance, by Kuhn in his account of normal scientific activity. In the terms of this thesis, as positive categories expand, this increases scientists' scope to deal successfully with the impact of the material world. The form of theoretical progress here is relatively unproblematic; by its own standards, a theory is progressing if it can systematically account for more and more phenomena in its domain.

The picture becomes more complicated when we imagine that there are two different theories which share the same standards, but have conflicting substantive claims. In such a case, we want to be able to decide which theory offers the more resourceful interaction with the world. Even if theoretical standards are shared between competitors there can still be difficulties in making a reasoned assessment of competing theories. This is because the substantive categories held by the competitors may be quite different to one another, as exemplified by the clash between Newtonian mechanics and special relativity. The problem faced by scientists is how to manufacture neutral tests of the theories that will allow us to distinguish between the adequacy of two sets of understandings. It is this kind of difficulty that Kuhn identifies when he argues that scientists who employ competing theories operate in 'different worlds' populated by different substantive entities and processes (Kuhn, 1970: 150). Logical positivists would suggest that disputes could be resolved by using a foundational observation language to establish which theory was in line with the observational 'facts' of the matter. As we have seen above, the idea that such an observation

language exists has been strongly challenged, in part because of Kuhn's work²¹. We are then left with a problem of how to resolve disputes between scientists operating in different worlds²².

The first thing to say about this situation is to underline the simple but fundamental point that for there to be an issue of reasoned debate here, there must be real conflict and competition at the substantive level and not mere difference. For example, molecular biology and astrophysics postulate different kinds of entities in their theories. However, their theories are not in conflict with one another, because they are not making different claims about the same subject matter; there is no theoretical disagreement which they are attempting to rationally settle. As Bhaskar usefully puts it:

Special relativity is in conflict with Newtonian mechanics, but not the Battle of Hastings, Lymeswold cheese or the rules of chess, although these are all undeniably different. (Bhaskar, 1986: 79)

Thus, we must not confuse situations of theoretical difference, where it does not make sense to talk of a reasoned assessment of the superiority of one view to the other, with those of competition, where disagreement may be productively resolved. The failure to explain how we might reasonably decide between 'different' theories provides no support for the relativist. Conversely, the inability to show how genuine disputes can be resolved gives weight to the relativist argument.

The problem of resolving disputes between theories with quite different substantive categories is not as difficult as it may seem. Theories which compete with one another are in agreement that they, at some level, deal with the same phenomena. This agreement is the shared premise of the theories, and is what indicates that they are competing and not

²¹ It is worth noting that Bhaskar's solution to the incommensurability problem is based on a similar attempt to find a non-theoretical ground for theory communication and comparison. He locates this in the realm of ontology, arguing that theoretical debate is only intelligible if theories refer to the same intransitive objects (Bhaskar, 1986: 70-80). However, this runs into problems in relation to apparently meaningful debates over objects which are held by later theories to be non-existent, such as 'ether'. As Holmwood and Stewart argue, Bhaskar's approach displays the ontological realist tendency to project current theoretical categories into the ontological level, rather than accepting that these categories may be totally transformed by theoretical development (Holmwood and Stewart, 1991: 18-9).

²² I have set aside issues of 'communication' here in favour of focusing on issues of 'comparison'. The remarks that Kuhn makes about strict problems of communication are rather banal. He suggests that the same term may have a different meaning for different paradigms, and this can be a source of confusion (Kuhn, 1970: 149). It is hard to imagine that linguistic confusions over the meaning of terms like 'earth' cause continuing difficulties in communication, just as there is only a limited amount of misunderstanding likely when I use the term 'plant' as both a noun and verb. The problems indicated by Kuhn are really problems with grasping conceptual innovation, for example shifting from an unquestioned presupposition that the 'earth' is fixed, to the claim that the 'earth' moves.

complementary. The dispute results from the fact that, at the present time, different conclusions can justifiably be drawn from the premises that are agreed upon. To decide between the theories, tests must be set up which discriminate between the adequacy of the different conclusions postulated. The existence of different conclusions will allow us to test the theories, in order to see which line of thought provides the better account of experimental outcomes. One potential block to producing decisive test-results is that theories may describe the world in different ways, and thus describe the test-results differently. However, as Hesse points out, in order to test theories we do not need to find an observation language that is neutral for all time, just a way of characterising the results of tests which is neutral between the two theories, and which both sets of scientists can agree will discriminate between the theories (see Chpt.1 of Hesse, 1974). Once we have worked out the theoretical grounds which can neutrally characterise the dispute, we can go about designing a test that will distinguish between the predictions made by the competing theories. It may take time and ingenuity in order to devise such a test, and it may be the case that on our current capability we cannot carry out this test because of limitations elsewhere in our knowledge or available instrumentation. However, this does not provide support for the relativist position, as relativists must suggest in principle that theories cannot be distinguished, not that we may be unable to distinguish theories until we can gather the appropriate evidence. If we can state what outcomes would distinguish the superiority of one theory from another, then we have avoided relativism, although our actual capacity to make the decision may not yet have been produced.

Hesse constructs a useful example of how a conflict between theories with different substantive ontologies can be resolved (Hesse, 1974: 35-7). In this example, the goal is to distinguish between Anaximenes' and Aristotle's theories of fall. For Anaximenes, the universe has a preferred direction of fall, parallel and perpendicular to the surface of the disc-shaped world on which Greece is situated. For Aristotle, items fall toward the centre of the (spherical) world, always travelling in line with radii extending from the centre of this sphere. As Hesse notes, the concept of 'fall' is theoretically loaded and somewhat distinct for each thinker. For Anaximenes it refers to a particular direction of travel that is uniform throughout space. Conversely, for Aristotle, it refers to motion towards the centre of the earth along the lines of radii from that point. Hesse suggests, however, that even given their substantive theoretical divergence, it is possible to design an experiment to distinguish between these theories. What is required is that shared presuppositions can be employed in

order to generate a neutral test-situation that does not favour one theory or other, but the result of which will distinguish them.

The experiment would go as follows: Anaximenes and Aristotle are blindfolded and taken to the other side of the earth (the blindfold is necessary to avoid predistinguishing the theories based on the shape of the earth). They can agree that they are on the other side of the world given the position of the stars, this being a presupposition shared by both theories. They can also agree upon how to describe the motion of a stone that is released (it travels towards the earth or away from it). However, each expects that when a stone is released it will travel in a different direction. Anaximenes believes that it will travel away from the surface of the earth (in line with the universe's direction of fall), and Aristotle believes that it will travel towards the surface of the earth (along the line of a radii from the centre). As we now know, if the experiment was conducted the stone would travel towards the earth, suggesting the superiority of Aristotle's account.

We have seen here that it is possible to distinguish substantively different theories by manufacturing a situation where they have competing expectations about some event. The experiment does not rely upon a neutral observation language, but only a means of describing the experimental situation and outcome which both participants can agree upon (in the above example this involves factors like the position of the experimenters, and whether a stone is moving towards the surface of the earth or away from it). Of course, the theorists have different substantive expectations, but the experiment is designed so that both can accept that their conflicting expectations are being tested in a neutral manner.

Translating the experimental result into critical historicist terms, the successful theory demonstrates the power of its positive categories by being able to systematically account for a new test result. Likewise, the unsuccessful theory has a new anomaly to deal with, which remains a problem for the theory, and interactions with the world using that theory, until it is given a systematic account. In relation to this problem, one theory has proved more resourceful than another.

This cannot be the end of the matter, however. Extrapolating from arguments made by Quine in 'Two Dogmas of Empiricism' (1953), some thinkers have argued that theoretical tests of the kind discussed above cannot be used to distinguish the success of theories in the way I have defended above. The basis of this claim is the 'underdetermination' thesis, which argues that the evidence that we gain from tests always underdetermines the theoretical

consequences that we take from it. In its strong formulation²³, it could be argued that the outcome of a test cannot be used to distinguish which theory is more practically successful at dealing with some particular phenomenon. I shall consider this strong formulation here, as it provides the main challenge to the account given above²⁴.

The argument for strong underdetermination goes as follows. When new evidence is presented with which a specific theory is inconsistent (say that produced by a recently conducted experiment), it is not the case that this simply demonstrates the lack of resources of the theory to deal with this evidence. This is because what is being challenged is not simply one theory but a whole web of theories and auxiliary information which all play some part in producing the relevant theoretical expectation. In other words, the character of theory testing is holistic. The understandings employed in tests can range from things like optical theories, to theories of other domains, to basic presuppositions that evidence collectors were reliable and were not hallucinating or otherwise operating suboptimally. Crucially, in the face of recalcitrant evidence, we may alter a part of our understanding which is not directly connected to the specific theory in question. So, to take an example mentioned above, if the observed location of the moon does not fit with a particular gravitational theory, the scientist may retain the gravitational theory and challenge the observational theory which was used to locate the moon.

The strong underdetermination argument generalises from this possibility to argue that by making alterations to other understandings and auxiliaries, any theory can give an equally systematic account of some particular set of evidence. Whereas I argued above that evidence can be used to discriminate the resourcefulness of theories, the underdetermination argument claims that this is not the case. Apparently recalcitrant evidence can be given a systematic account within any theory by making the appropriate adjustments elsewhere. Whatever evidence that one might produce can be equally well accounted for by competing modes of understanding, leading to relativist conclusions about the possibility of discriminating theoretical validity.

Larry Laudan identifies what is wrong with this argument. It is certainly true that by making appropriate adjustments, one can stop a body of theory from clashing with a

²³ Laudan (1996) usefully clarifies different forms of the underdetermination argument and suggests the need to distinguish the more conservative and defensible forms from those tending to relativism.

²⁴ Weaker forms of the underdetermination thesis might suggest that the evidence derived from tests does not uniquely support any one particular theory. However, this does not mean that carefully chosen tests cannot distinguish the resourcefulness of some two specific theories that are currently held to be plausible. That is to say, although evidence may not support one theory uniquely, this does not mean that it supports every theory equally (see for discussion Laudan, 1996: 41-2).

particular set of evidence. However, there is no reason to think that the resulting body of belief will be as resourceful as any of its competitors (Laudan, 1996: 36-8). After all, in order to avoid contradicting the evidence it may well be necessary to sacrifice a whole set of beliefs that were resourceful in dealing with a certain area, and for which there is no replacement.

As an example of this process we can consider the rejection of an observational theory which is blamed for the lack of fit between evidence and a general theory. A scientist argues that the observational theory should be rejected, and is the source of the inconsistency. Nevertheless, unless a good alternative observational theory is produced, then this scientist loses all the resources of systematic categorisation that the initial theory had, and decreases his or her ability to deal successfully with the domain covered by it. Of course, as is the case in the Newton-Flamsteed interaction, it may well be that the scientist does have a systematic alternative conception, and that this expands the resourcefulness of the original. However, this theoretical success cannot be assumed, and, like any theorising, requires work and creative insight in order to be achieved.

The real issue here, then, is not to avoid clashing with a set of evidence, but to be able to produce a consistent account of it. Producing a consistent and thus resourceful account is not a simple or unproblematic process. While it may be possible to preserve the resources of part of a theory by altering understandings elsewhere, without serious labour and theoretical success, alterations are likely to decrease the general resourcefulness of the understandings employed. Certainly, the most simple alterations that will avoid a clash with evidence are those which simply hive off the capacity of some part of the general theoretical network. Thus, emphasising the holistic character of theory testing does not mean that the adequacy of theories can always be preserved regardless of the evidence. Rather it means that it is possible to trade off the resourcefulness of one part of the theory so that (until further evidence arrives) the adequacy of another part is preserved. This still gives us grounds for wider theory comparisons based upon the general resourcefulness of each theory complex.

The general underdetermination argument is pressing because it states that when we compare any two theories with available evidence, we cannot distinguish between their adequacy in accounting for this evidence. It should be noted that even if a theory which initially clashes with evidence can be reconstructed in order to become consistent with it, this does not establish underdetermination. This is because the evidence made it necessary for theoretical reconstruction to occur, so that we are now assessing a new theory, even if part of it has remained unchanged. Furthermore, unless this reconstruction has proved to be equally

as resourceful as the theory complex that it is competing with, then we still have grounds for favouring its competitor. For these reasons, the strong underdetermination argument is unconvincing.

This concludes our examination of the most simple types of theoretical assessment, which has raised three important points. Firstly, we must be very careful to distinguish mere difference from conflict and competition. There is no reason to think that we can or should be able to rationally decide which of two different theories is superior to the other. As such, the crucial point for relativism is to establish that we cannot rationally decide between theories that are in conflict with one another. Secondly, when comparing theories, our understanding that they conflict with one another provides the basis for generating tests that will distinguish the adequacy of one theory from another. We do not need to step outside of these theories in order to do this, but instead locate the overlap between theories and use this as the testing ground. It seems that the production of such tests is possible, undermining the idea that some degree of object-level incommensurability leads to relativism. Thirdly, the testing of theories is holistic, but this does not mean that we cannot make valid assessments regarding which theory complex is currently the most resourceful, and deals most successfully with particular problems.

It is now time to turn to more complicated cases of theory comparison. So far we have considered only comparisons where substantive matters were at issue. However, our understandings do not merely consist of substantive theoretical elements, but also contain certain standards which delineate what is a valuable and productive approach to gaining understanding from what is not. These standards may be low-level methodological rules, such as those governing the proper use of statistical data, or outlining how a particular type of experiment should be undertaken. Alternatively, they may be higher-level rules such as those delineating good evidence from bad, or limitations on what forms of explanation are legitimate.

The importance of these standards for considering issues of progress is emphasised by Kuhn in *The Structure of Scientific Revolutions*. He suggests that science does progress within the framework of a paradigm, whereby understandings are being developed in line with a certain set of standards governing what is or is not a permissible move to make. However, when we are assessing which of two competing paradigms offers the more resourceful approach to some domain, there are serious difficulties with making a

judgement²⁵. This is because the very standards which we employ to judge the superiority of an approach vary between paradigms (Kuhn, 1970: 103-9). Each paradigm has its own methods, problems, and standards of solution, and changing from one paradigm to another involves a shift in these elements. In relation to paradigm succession, Kuhn argues that

...as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play. The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before. (Kuhn, 1970: 103)

This raises new difficulties for theoretical comparison. I argued that in the simplest case, we can say that a mode of interacting with the world progresses as it extends its ability to classify the world using the concepts and standards that it employs. However, when it comes to comparing theories, we may find that they have different standards as to what is good and bad practice. In such a situation, according to Kuhn, it is not possible to make a judgement as to which theory is superior. This applies whether we are comparing the superiority of predecessor and successor paradigms in the history of some particular field, or attempting to decide at present which of two or more competing paradigms is more resourceful for dealing with the domain in question.

It is this state of affairs that leads Doppelt, in his useful commentary on *The Structure of Scientific Revolutions*, to refer to standards of judgement as 'normative' (1978: 41). By this he means that standards have an irreducible element of what 'ought' to be done. Decisions about which standards to employ, and thus which paradigm to pursue, are questions of value, and not assessable by rational means. If Kuhn is correct, suggests Doppelt, we can compare scientific conflict to conflict in ethical and political life²⁶ (1978: 41). Conflicts may contain elements of rationality, but the final decision is a choice between competing values which allows of no rational resolution²⁷. We can legitimately ask, however, whether Kuhn is correct.

²⁵ The incommensurability of paradigms at the level of objects was frequently taken to be the most important problem raised by Kuhn for rational accounts of theory change (see for example, Laudan (1976); Shapere (1984 [1964])). The fact that Kuhn himself supported this view helps to explain its prevalence (Kuhn, 1970: 150). However, Doppelt has argued that this was somewhat of a minor issue, and that Kuhn himself relied upon their being some degree of object-level comparability between paradigms (Doppelt, 1978). Rather, suggests Doppelt, the feature of incommensurability that supports relativism is the differences between paradigms at the level of standards.

²⁶ This connection is already suggested by Kuhn in *The Structure of Scientific Revolutions* when he parallels scientific revolutions with those of a political nature (Kuhn, 1970: 92-4).

²⁷ There is no evidence that Doppelt is thinking of any particular sociological position here, but his account has a strongly Weberian ring.

In response to some of the philosophical problems arising from the 'shifting standards' thesis, Laudan and Shapere have attempted to reconstruct how we conceive of the standards governing scientific enquiry. For Kuhn and Doppelt, these standards are monadic, ultimate and self-contained. In this sense, their thinking connects with an older philosophical tradition in which standards of reasoning are necessary truths which have no need of empirical justification. Their twist to this tradition is to retain the formal, non-empirical status of standards of reason, but emphasise their historical variability and the lack of means to decide between alternatives. In contrast, Laudan (1981b) and Shapere (1984) make a crucial contribution to critical historicism by arguing that scientific standards are as empirical as any other aspect of science, and open to testing and acceptance or rejection depending on how successful they are as standards. On this view, as science develops, so do the rules that govern the legitimacy or otherwise of its forms of activity. Most philosophers accept that by pursuing scientific activity we gain substantive understanding about the world. Critical historicism would add that through scientific activity we also learn about reasoning, and come to understand which forms of reasoning are practically reliable, and which are not. As such, when we get into situations where modes of scientific activity clash with one another, it may be the case that the competition between these modes is not only over which has the better substantive understanding, but which uses the better rules of reasoning.

One way to bring out the empirical character of standards is to parallel them with the substantive understandings that we gain through science. Our best substantive theories provide us with the most systematic understanding of how to deal with entities and processes in the world. Likewise, our best scientific standards provide us with the most systematic understanding of the reasoning that we use when interacting with the world. These standards are a repository of information about which modes of reasoning are more or less successful. In this way, scientific standards are as empirical as substantive claims and should be treated as such. Standards can and should be judged by their empirical and conceptual grasp of modes of reasoning, and their ability to discriminate successful and unsuccessful methods of interacting with the world. As with substantive claims, this cannot be done by comparing our understandings with the external world. Instead, evaluating standards involves judging their (internal) ability to systematically deal with our reasoning processes.

Laudan offers a useful example of how a new scientific standard arises through connections with empirical investigation (Laudan, 1984: 38-9). In the field of pharmacological testing, scientists used to test a drug by dividing patients into two groups, giving one group the drug, and the other group nothing. If the first set of patients reported a

higher rate of recovery than the second, it was concluded that the drug was effective. However, this methodological approach was disrupted by the realisation that there was a 'placebo effect' in operation. That is to say, patients seemed over-responsive to the drug administered, and it was discovered that the subjects' belief that they were taking a course of medicine could induce a recovery, no matter whether the medicine was effective or inert. As such, single-blind methodology had to be introduced, whereby all patients are administered with pills, and they do not know whether they have received the drug or a placebo. This methodology controls for the expectations of the subjects.

The development of single-blind methodology exemplifies certain features of the development of scientific standards. The standard was produced by reflecting upon actual practice, and what had been learned in the process of experimentation. It acts as a repository of understandings about experimental practice, and the problems and advantages of some kinds of experiment. Furthermore, because it can distinguish successful from unsuccessful investigation, this provides a justification for its use. It is fairly clear, then, that single-blind methodology is not a 'value' that cannot be rationally reflected upon. Rather it is a claim, backed up by empirical evidence, about how to successfully investigate some range of phenomena. As with any empirical claim, it is not certain or guaranteed, but in the absence of a competitor which can successfully dispute its claims, it is a justified methodological standard²⁸.

Not everyone subscribes to this critical historicist account, however. Worrall (1989) argues that although the 'empirical' approach to scientific standards may apply to low-level methodological rules, it cannot work for higher-level scientific standards that make up the 'core' of scientific rationality. His argument is that although low-level methodological rules do change in relation to empirical discoveries, the logic behind these changes must involve higher-level, stable standards of reasoning. In the example of single-blind methodology Worrall suggests that the development of this standard can only have been rationally defensible if it conformed with a fixed, higher-level standard of reasoning, along the lines of "when testing hypotheses, shield the experiment against other possible causal factors". So, although it was true that the specific content of the methodological rule was related to empirical investigation, the form of reasoning involved was neither empirical nor subject to alteration. In order to make reasoned judgements about standards we must be using some inflexible, non-empirical principles which provide the ultimate basis for these judgements.

²⁸ It is worth noting that drug-testing methodology has developed on from the single-blind approach, as it was discovered that the expectations of experimenters also had some influence on the test results. This led to the development of double-blind methodology (Laudan, 1984: 39).

To assess Worrall's arguments, I shall discuss two debates around high-level rules of reasoning employed in scientific activity, and examine their character and rationality. For reasoned outcomes to take place there need only be a general commitment to consistency, which I shall suggest is a general standard of reasoning of an unusual kind. With this commitment in place, we can argue that certain changes in standards can be rationally justified.

The debate around the status of falsification as a rule of theory choice has been ongoing since Popper's *The Logic of Scientific Discovery* (1959 [1934])²⁹. I will rather brutally cull out some of the main falsificationist ideas in order to set up the position to be discussed. The impetus behind falsificationism is that the problem of induction is irresolvable, meaning that any attempt to justify theories inductively must fail, including probabilistic variants (Popper, 1959: 27-30). Therefore, the only valid approach to theory testing is a deductive one, in which theories and auxiliary information are mobilised to deduce certain consequences or predictions. If such predictions clash with accepted basic statements then the theory can be considered falsified, and scientists must search for an improved theory which would not be falsified by existing evidence, and does not deal with this evidence in an *ad hoc* manner. We should also favour daring theories with a large empirical scope, because these are more easily falsified, and we learn more from them if they survive criticism. If a theory survives rigorous testing, this gives no warrant for assuming its (probable) truth, but merely means that it has survived tests where other theories have failed.

This sketch of falsification does nothing to capture its interest and subtlety. However, even from this limited account, we can see that it is possible to have a rational discussion about the falsificationist standard. Firstly, the standard cannot be construed as a monadic 'rule of reason' but is closely related to a variety of debates and issues. For example, it is intended to avoid the difficulties with the problem of induction. However, if an appropriate resolution to the problem of induction was found, then induction would provide a superior mode of justification, as it would not only allow us to know which theories were false but also which were the most plausible. Secondly, in the 'strong' version of falsificationism, a theory must be rejected if it is falsified by relevant evidence. However, as Lakatos has argued, many theories which became successful by falsificationist standards started off with serious and obvious falsifiers (Lakatos, 1978). This includes both Copernicus' and

²⁹ In actual fact, Popper discusses falsification in respect to demarcation and not methodology, but the debate can be taken into the methodological realm.

Newton's theories which ended up with large empirical scope and dealt successfully with many potential falsifiers. Therefore, our knowledge of the historical development of theories, and the need to shield theories from early falsification, suggests that the strong falsificationist position must be revised. We should note that neither of these could be seen as 'external' criticisms of falsificationism using a higher methodological standard. Rather, arguments that have been used to make falsification plausible and appealing are examined in order to see whether they are defensible on their own terms. The plausibility of falsificationism rests on a body of empirical and conceptual argument that can be reflected upon in order to support or challenge its tenets.

This example remains somewhat abstract because the standard examined was put forward by a philosopher in a mode somewhat divorced from scientific practice. A more practical scientific example is the debate around 'the method of hypothesis' (Laudan, 1984: 55-60). During the 18th century, postulating hypothetical entities was considered improper, and it was argued that scientists should not theorise about that which could not be directly observed. However, by the 1830s, many scientific theories postulated such entities, leading scientists to argue for the legitimacy of a 'method of hypothesis'. Although strict inductivism from experimental data was the dominant standard of reasoning, a number of scientific areas could not be successfully investigated in this way, including electricity, embryology and chemistry. Some scientists investigating these areas set about to argue the legitimacy of the method of hypothesis, indicating that many successful theories of the time (such as Franklin's theory of electricity and Boerhaave's theory of heat) relied upon hypothetical entities. They argued that although hypothetical reasoning could not produce theoretical certainty, this was also true of other valid approaches such as inductivism. It was further claimed that even theories ostensibly produced by induction alone always generalised beyond their premises, and include a hypothetical element. The proponents of the method of hypothesis won the day, and swayed the scientific community to the idea that hypothetical reasoning could be scientifically productive.

Again, we can enquire into the rationality of this process. A relativist could claim that this change was merely a shift in 'values', from the value of strict induction to the value of hypothesis, with no rational grounds for choosing between them. It could be argued that each was a self-contained standard of reasoning in its own right, and that there could thus be no 'reasonable' choice between the two. I would dispute this by pointing out that standards have their own rationales, and their own implicit or explicit presuppositions which are used to provide justification.

In the case of strict inductivism, one implicit claim is that theories that are not generated by induction from observation are unreliable, that is, turn out to be spurious or misleading on further investigation. It is also implicit that general theories can actually be produced using strict inductive methods. On this basis, we can see that inductivism is not an isolated standard of knowledge, but bound up with conceptual and empirical claims, which may be supported or challenged by further investigation. It is this kind of investigation that goes on when another standard, such as the method of hypothesis, is put forward as an alternative.

This comparison of two different standards should be viewed in exactly the same way as a clash of substantive theories. It is possible that two different standards are intended to measure quite different things (such as simplicity and empirical adequacy) and that they are not in competition with one another³⁰. The failure to choose between different and non-competing standards does not give support to the incommensurability argument. On the other hand, it may be that there is a genuine competition between the standards, and that they dispute the adequacy of some mode of reasoning. In such cases, we must identify the competitive overlap between them, and as with substantive disagreements, find a mutually agreeable testing ground to compare the adequacy of the theories.

To return to the clash of strict inductivism and the method of hypothesis, proponents of the latter scrutinised the conceptual rigour of the former, and suggested that important and reliable theories such as those of Newton involved a degree of hypothetical reasoning. They also suggested that hypotheses could be reliable to some degree, and that the continuing ability of hypothetical theories to meet other scientific criteria demonstrated this. They thus provided conceptual and empirical support for the plausibility of the method of hypothesis, which came to be accepted as a rational scientific standard.

In general, then, critical historicism suggests that scientific standards are neither arbitrary matters of choice, nor fixed as some eternal set of standards of reason. Rather they are claims about the best ways to interact with the world, which are supported by various kinds of arguments and evidence. Criticism of a standard involves reflecting on these claims and their adequacy, using other substantive knowledge and standards of reasoning. Just as with substantive beliefs, it is rarely or never the case that a significant set of standards is consistent on its own terms, when those terms are spelled out. Critical reflection can improve standards by making them more conceptually consistent, or by increasing their ability to discriminate between empirical cases. Because standards are supported by webs of

³⁰ Of course, different standards can be used in claims about the general adequacy of theories, in which case we need to treat them as competing and work properly through their competitive overlap.

reasoning and empirical claims, clashes between standards are in principle open to reasoned assessment. These clashes can be resolved by finding where the standards disagree and testing out the competing claims in a neutral manner, to discover which is most resourceful for discriminating successful practice.

The observant reader will have noticed that the discussions of the development of substantive theories and of standards have emphasised the value of consistency. Substantive progress is to be measured in large part by the systematic and coherent nature of the categories produced. Likewise, scientific standards are to be assessed by the extent to which they are internally consistent, consistent with other standards, and cohere with accepted empirical understandings. One could well ask: why must we value consistency so highly? Couldn't we reject consistency as an important part of theoretical development? If this was genuinely the case, then the methods of developing understandings and resolving conflicts proposed here would carry little or no weight, predicated as they are upon the value of consistency.

However, it is a mistake to think that consistency can simply be rejected as a theoretical value. I would argue that rather than being an optional standard, consistency is the implicit presupposition of all analytical understanding that cannot be rejected without rejecting what one regards highly. In other words, to say that someone is consistent is to say that they live up to their own standards, that is, the values and beliefs by which they orient their thinking. A lack of concern about consistency would involve a lack of concern for those things that one ostensibly approves of.

The force of these remarks is most apparent when we connect conceptual adequacy with practical activity, and the need to deal successfully with the material world. As Holmwood (1996) has argued, consistency is important because it entails practical resourcefulness on the part of actors. If actors employ contradictory beliefs in an activity, then they are in a state of 'practical alienation' in which the contradiction results in outcomes which are not those intended by the actor (Holmwood, 1996: 115-6). Thus failure to be consistent is failure to achieve the outcomes one desires. The process of producing consistency is nothing more than the process of becoming resourceful. Our postulated theories and standards are tested out in their application, producing positive and residual categories. The inconsistencies that result force us into a process of reconstruction, if we are to have smoother and more predictable interactions with the environment. Some reconstructions will be driven by comparison with other theories, which may demonstrate how certain problems can be tackled more effectively. Others will be produced by reflection on a theory and its

strengths and weaknesses. In these ways, theories are pushed forward, developing greater resourcefulness and consistency in the process.

It is formally possible to reject improvements to a set of beliefs which resolve contradictions within one's own understandings and produce greater consistency and success on one's own terms. But to genuinely do this (rather than merely doing so in theory but not in practice) is a form of self-denial which is destructive of the capacity to act. It is to accept the pressures of the material world which disrupt one's claims but not to respond by increasing capacity and consistency.

This is why consistency is a special kind of theoretical standard. It is not an imposition from outside of thought, but a description of the nature of successful understanding on its own terms. Combining this claim with the idea that the material world resists scientists' understandings provides the basis for a historical approach to scientific understanding. All elements of scientific practice are up for reflection and reconstruction as science develops, and as theories attempt to deal with their problems of application. The general process of increasing consistency is the process whereby standards and substantive theories become more capable and resourceful at dealing with the world on their own terms. As long as we can see that new standards and theories are connected to past understandings by their more resourceful approaches to earlier problems, we can argue that theories have progressed by increasing their consistency. Of course, new standards and theories also generate new problems, necessitating further developments and reconstructions.

1.6 Conclusion: from science to social life

In this chapter, I have defended a 'critical historicist' view of science, which provides an alternative to both foundationalist and relativist positions. In contrast to foundationalism, critical historicism argues that natural scientific investigation has no special epistemological privilege based on its relation to its object, or its mode of reasoning. On the question of the knowledge-object relation, it suggests that we have no direct and unchallengeable knowledge of the world. Furthermore, because of the mediated character of our understandings we are never in a position to make an inference from our theories to claims about the nature of the world in itself. Attempts to do so by claiming the truth of theories or their relation to 'real structures' fix knowledge at its current level when further reconstruction may be required. Instead, theories should be viewed as more or less coherent accounts of events in the world, which, when successful, make those events intelligible and predictable. Likewise, there is no special mode of scientific reasoning which guarantees the validity of natural scientific

knowledge. Rather, modes of reasoning develop as science develops, and are repositories for what is learned about inquiry as scientific investigation continues. This emphasis on learning and the development of coherent theories through the course of investigation distinguishes critical historicism from relativist accounts of science. Although science has no foundations, it is not the case that all theories are equally valid. Theories do not unproblematically construct the world 'on their own terms', instead having both systematic categories and anomalies, the latter of which demonstrate the existence of theoretical difficulties and confusion. Comparisons can be made of the extent to which theories render interactions with the world coherent, and a theory may be able to show that what is anomalous for a competitor can be coherently accounted for within its own framework. Thus, contrary to relativism, the resourcefulness of theories can be assessed, and success is something that has to be strived for by reconstructing understandings in order to produce coherence.

This account of science is important to sociology because it challenges the common view that there is a division between natural scientific investigation and other forms of social activity. As we shall see, one version of this dualism focuses on issues of success and meaning. It is argued that the success of natural scientific theories is achieved by some direct apprehension which is non-social in character. Other social practices are then differentiated from science on the basis that they are 'meaningful', with the consequence that success is not an issue in the same way. The arguments of this chapter undermine such a separation. On the one hand, the rejection of foundationalism suggests that natural scientific theories are never 'direct', but always theoretically mediated. As such, they are constituted by meanings in the same way as other social practices. On the other hand, it is crucial to note that the mediated nature of theories does not remove questions of variable success from consideration. This insight can be taken into the analysis of other social practices, suggesting that their success can be gauged in the same way as natural scientific beliefs. As with natural scientific theories, social practices are meaningfully constituted attempts to interact successfully with the world. These meanings may be more or less coherent, producing some expected results, and other, anomalous, outcomes. Thus, the critical historicist argument is that both natural scientific investigation and other social practices are meaningfully constituted and variably successful.

The other important version of the science/society dualism suggests that, in science, beliefs are defended because of their rationally accountable success, whereas, in society, beliefs may be defended because it is in the strategic interests of a social group to do so. The

implication of this division is that it can be in the interest of a group to defend certain claims even if they run contrary to the claims produced by science. Although I have drawn on some of the ideas of Lakatos and Laudan to help challenge the science/society division, they themselves accept this division, distinguishing internal and external accounts of scientific development (see Lakatos, 1978; Laudan, 1977). On this approach, the theoretical decisions made by scientists are split into those which can be accounted for rationally (internally), and those which can only be accounted for using non-rational (external) factors, which are paradigmatically social ones. Although they consider scientific investigation to be largely rational in character, Lakatos and Laudan accept that social interests may sometimes disrupt reasoned debate. In such cases, a group may defend a theory because it is in their interest to do so, even if this theory clashes with rationally justified scientific knowledge.

Versions of this argument will be considered in more detail later in the thesis, particular in the discussions of realism (Chapter 3) and social constructionism (Chapter 4). For now, I want to indicate that critical historicism removes the need to place scientific analysis of the variable success of beliefs and the impact of social interests in tension with one another. Socially interested activity is oriented to achieving a certain outcome, but this requires knowledge, and is more or less successful depending on the adequacy of this knowledge. As science is a way of investigating which knowledge allows successful interactions with the environment, it is not antithetical to 'interests' but is a mode of pursuing these and reflecting on their success. For a group to have an 'interest' in contradicting scientific knowledge, would be for them to have an interest in being unsuccessful in their relations with the environment. In the remainder of this thesis, I hope to demonstrate that this is the case, and show that the critical historicist position can be used to avoid the problems generated by the separation of science and society.

Part II

Science and Society Divided: Contemporary Sociological Theory

2

Antinaturalism in Sociology: Structuration Theory and 'Meaning'

2.1 Introduction

The idea that social life has extra characteristics of 'meaning' over and above those that are found in natural scientific investigation has a long history within sociology. Max Weber, one of the founders of the sociological tradition, makes such a claim in his typology of social action (Weber, 1968: 24-6). One type of action, that which is instrumentally rational, is 'determined by expectations as to the behavior of objects in the environment' where these are means for securing the ends desired by actors (Weber, 1968: 24). Such action exemplifies the technical orientation of natural science. In contrast, 'value-rational' action has a different orientation, being concerned with the pursuit of certain meanings, that is, values, which are outside the bounds of scientific reflection. As Holmwood and Stewart note, although Weber's approach to this matter is usually held to be unsatisfactory, the idea that social life is meaningful in a way distinct to natural science is commonly accepted in sociology, and is to be found in the work of Parsons, Giddens, and Habermas, among others¹ (Holmwood and Stewart, 1991).

The claim that social life involves extra issues of 'meaning' not present in natural science invokes a division between the two forms of activity. In this chapter, I want to consider the confusions generated by this notion, and the consequences for our understanding of social science. In order to do so, I focus on the work of Anthony Giddens, whose structuration theory is a prominent example of the approach criticised here. Giddens separates the 'social'

¹ For example, in *The Structure of Social Action* Parsons defines the 'unit act', the basic analytical element required to describe social action (Parsons, 1949 [1937]: 43-51). This act involves both: (i) an initiation in the 'situation' of action, which is those aspects that have to be dealt with using means/end (i.e. scientific) rationality; and (ii) a directedness to 'ends' which are independent of this situation, and thus undetermined by scientific rationality. The existence of the latter element is what distinguishes social action from the purely technical responses to the environment provided by science. Likewise, Habermas' early work distinguishes between two forms of action, 'instrumental' action that is oriented to a successful relation with the environment using scientific knowledge, and 'communicative' action which is grounded in intersubjective agreement rather than succeeding or failing by its ability to produce desired outcomes in interactions with the environment (Habermas, 1971).

and 'natural' realms on the basis that the natural world is made up of mechanical, meaningless occurrences, whereas the social world is constructed out of 'meanings' and intentional activity. From this, he argues for an anti-naturalistic conception of social science, that is, one that distinguishes social science from natural scientific investigation. According to Giddens, the 'meaningful' nature of social life implies three kinds of differences between natural and social science. Firstly, social science involves a 'double hermeneutic' where there is not simply a relation between theory and object world (as in the single hermeneutic of natural science) but a relation between a social scientific theory and actors' understandings (giving two layers of meaning and the double hermeneutic). Secondly, because the 'social world' is constituted by meanings, there is a special connection between social scientific theory and its subject matter. Whereas natural scientific theories stand outside their object world and have a technical relation to it, social scientific theories may be taken up by social actors, and thus come to constitute the world that these theories describe. Thirdly, actors in the social world do not obey the iron laws of mechanism found in the natural world. Because social action is 'meaningful' it involves possibilities of freedom and choice which do not exist for natural objects. Social scientists must take account of this potential in their theories, whereas natural scientists have no such concerns.

In the course of this chapter I shall suggest that each of these arguments is problematic and, ultimately, unsustainable. I draw on John Holmwood's work to argue that the 'meaning' involved in social activities should be directly paralleled with that in natural scientific investigations, as the meanings of both are oriented to interacting successfully with the world (Holmwood, 1996). As such, there are issues of meaning in social life, but these are no different to the issues of meaning present within natural science.

2.2 The double hermeneutic and actors' knowledge

The idea that social science involves a 'double hermeneutic' is widely accepted within social theory. Although Giddens was by no means the first to argue for the special status of social science because of issues of meaning, his reference to these as the 'double hermeneutic' (a term coined in *New Rules of Sociological Method*) has become standard usage. Thinkers such as Habermas (1984: 109-11), Bohman (1991: 106) and Outhwaite (1987: 76, 70) all argue for the relevance of the 'double hermeneutic' within social science, although they do not necessarily agree fully with the conclusions that Giddens derives from it.

The argument for a 'double hermeneutic' in social scientific inquiry is based upon the claim that the 'social world' has characteristics that make it fundamentally distinct from the 'natural world'. Giddens states:

The difference between the social and natural world is that the latter does not constitute itself as 'meaningful': the meanings it has are produced by men in the course of their practical life, and as a consequence of their endeavours to understand or explain it for themselves. Social life - of which these endeavours are a part - on the other hand, is *produced* by its component actors precisely in terms of their active constitution and reconstitution of frames of meaning whereby they organize their experience. (Giddens, 1976: 79. Author's emphasis)

Giddens thus claims that unlike the natural world, society is constituted by the activities of human beings, who are its creators. Because of this provenance, the social world is made up of 'meanings', and social phenomena are inherently meaningful. As a result, investigations into this realm necessarily differ from those oriented to understanding the natural world. Natural science involves a single hermeneutic in which a pre-existing, meaningless object world is interpreted by scientific theorists attempting to successfully describe its character. By contrast, Giddens suggests that sociological analysis involves a double hermeneutic:

Sociology...deals with a universe which is already constituted within frames of meaning by social actors themselves, and reinterprets these within its own theoretical schemes, mediating ordinary and technical language. (Giddens, 1976: 162)

In other words, social science involves a theoretical description (one level of interpretation) of something that is already meaningful in itself (a second level of interpretation). Social science thus requires a double hermeneutic. Giddens believes that a range of consequences follow from this basic, ontological point, and these will be considered throughout the chapter. For the moment, however, I would like to reflect on the fruitfulness of using issues of 'meaning' to differentiate natural and social scientific enquiry.

As we have seen, Giddens accepts that natural scientific investigation necessarily involves interpretation, a point he takes to be established by post-positivists such as Kuhn (Giddens, 1976: 162). Furthermore, he is aware of debates around incommensurability which highlight the potential problems of understanding between natural scientists educated in different paradigms, that is different modes of interpreting the natural world (Giddens, 1976: 142-4). On this basis, it would have been quite reasonable for Giddens to argue that natural scientific investigation already raises issues of the 'double hermeneutic'. That is, when scientists from one paradigm try to understand the claims of those in another paradigm, they are attempting

to 'interpret' something that is already meaningful². In fact, it is hard to see how the 'double hermeneutic' can be avoided in accounts of natural scientific debate, given that debates are between scientists 'interpreting' the world differently. For these debates to be meaningful interchanges, each collective must understand not only their own theory, but that of their opponents, and thus engage in an attempt to comprehend a 'pre-interpreted' subject-matter. In Giddens' terms, this would involve a double hermeneutic. I would suggest that the only way to avoid this consequence is to revert to a non-interpretive position in relation to natural scientific discourse, and argue that scientific concepts are not 'interpretations' of the world but 'factual' statements guaranteed by their links with something like a non-corrigible observation language. This drastic move would not return us to Giddens' view of natural science as involving a single hermeneutic. Instead, it would remove issues of 'interpretation' from science altogether. As recent philosophy of science demonstrates that no such step can reasonably be taken, we should accept that natural scientific debate requires a double hermeneutic. This is not to accept that the consequences which Giddens derives from the 'double hermeneutic' in social science apply also to natural science. Rather, I shall be suggesting that as we examine these consequences, the parallels with natural science indicate the implausibility of Giddens' claims. Furthermore, by reflecting on issues of meaning in relation to natural scientific investigation, we can come to a more plausible account of these matters in relation to social science.

A close correlate to Giddens' argument for the 'double hermeneutic' is his emphasis on the knowledgeability of human agents. As Giddens puts it:

The production and reproduction of society...has to be treated as a skilled performance on the part of its members, not as merely a mechanical series of processes. (Giddens, 1976: 160. Author's emphasis.)

This is a feature of Giddens' structuration theory that is intended to differentiate his work from objectivistic social science, which, he contends, has ignored the fact that social actors are always knowledgeable. For Giddens, this knowledge is carried predominantly in 'practical consciousness', a repository of the skills and abilities possessed by an actor which they may not be capable of discursively expressing (Giddens, 1979: 57; Giddens, 1984: 6-8). These points suggest a contrast between social and natural scientific investigation. Whereas natural scientists study a preconstituted object world, social scientists study a world made up of knowledgeable activities, a world created by the skilful productions of actors.

² The quote above (Giddens, 1976: 79) also shows an awareness that natural science is a 'social activity' which should surely imply that understanding it involves a double hermeneutic.

It is not clear that this distinction is a useful or sustainable one. The argument of this thesis is that natural scientific theories are not best thought of as describing properties of an independent object world. Rather, I have suggested that natural scientific investigation is a sense-making activity in which theoretical constructions are tested for their coherence. The results of such tests are always to be understood as demonstrating the theoretical adequacy or otherwise of a set of understandings on their own terms. Learning a theory is thus not a matter of learning 'about' the object world, but of grasping a more or less coherent set of categories. It then seems reasonable to argue that debating and employing a natural scientific theory is a 'skilled performance' on the part of 'knowledgeable agents'. Much recent work within the sociology of science has emphasised this point, early developments being summarised in Knorr-Cetina (1983). Such work has also highlighted that natural scientific investigation is sustained by the employment of tacit skills not discursively acknowledged by participants (see for example Collins, 1985). One need not draw the relativist conclusions often found in these studies to accept that natural scientific theorising is a skilled and knowledgeable performance drawing on practical consciousness. In this sense, the worlds of natural scientists and the worlds of other social actors are both meaningful, knowledgeable constructions.

If this is true, are there still questions of meaning which might differentiate social and natural science? Giddens thinks so, and a central aspect of his antinaturalism is his argument that social scientific investigation generates problems of adequacy not faced by natural scientists. In natural science, any set of beliefs about the object world held by professional or lay actors is corrigible in the light of new findings. As Giddens puts it:

The natural sciences can in principle demonstrate that some of the things that the lay member of society believes about the object world are false, while others are valid. (Giddens, 1984: 335)

The validity of a natural scientific account is demonstrable by its superiority to competitors. In the social sciences, because of the double hermeneutic, gauging the validity of an account is not so straightforward. Giddens argues that because of its 'meaningful' subject matter, social scientific investigation must have two different modes, one of understanding and the other of criticism. When the social scientist is trying to grasp actors' meanings, these meanings are treated as 'mutual knowledge'; when social scientific criticism is being undertaken, actors' beliefs are treated as 'common sense'³ (Giddens, 1984: 336).

³ Other dichotomies introduced by Giddens such as 'sense' versus 'reference' (1976: 145) and 'credibility criteria' versus 'validity criteria' (1984: 339) are based on the same principle as the mutual knowledge/common sense division.

To take actors' meanings as 'mutual knowledge' is to insist that the social scientific observer methodologically brackets his/her scepticism. Actors' understandings must be analysed as if they were 'knowledge' rather than fallible 'belief' (Giddens, 1984: 336). In other words, actors' beliefs must be treated as fully adequate. For Giddens, this is a necessary consequence of the 'double hermeneutic', in that grasping a set of actors' understandings requires respect for their 'authenticity' and a suspension of doubt (Giddens, 1979: 251). As Giddens puts it:

Mutual knowledge, regarded as the necessary mode of gaining access to the 'subject matter' of social science, is not corrigible in the light of its findings; on the contrary, it is the condition of being able to come up with 'findings' at all. (Giddens, 1984: 336)

In other words, the social world is constituted by meanings, and to investigate it requires grasping those meanings by treating them as if they were fully justified⁴. Successful achievement of this is a prerequisite of an 'adequate' social scientific account in a way that has no correlate in natural science because the latter does not have a 'meaningful' domain⁵.

Once the hermeneutic task is fulfilled, however, Giddens suggests that social scientific investigation can take a critical standpoint. In this latter mode, the same understandings that had been viewed as 'mutual knowledge' are now to be treated as 'common sense'. This requires spelling out actors' understandings as propositions (where this is possible⁶) and treating them as 'fallible belief', that is, as corrigible (Giddens, 1984: 337). The empirical investigations of the social scientist are then directed towards an assessment of the truth or falsity of actors' beliefs. This allows social scientists to perform a critical task as well as a hermeneutical one.

Giddens illustrates the use of these modes in a discussion of the Zande witchcraft debate (Giddens, 1979: 252). Taken as 'mutual knowledge', Zande witchcraft rituals must be considered 'rational', being constructed out of 'internally coherent' frames of meaning

⁴ As we shall see in Chapter 4, Giddens' treatment of social beliefs as 'mutual knowledge' is not idiosyncratic. Constructionists such as Barnes and Bloor also argue that actors' beliefs must be viewed as 'self-validating', that is, fully adequate on their own terms.

⁵ It could, however, be argued that conceiving actors' categories as non-corrigible in the process of scientific investigation gives them a status similar to that of a foundational observation language in natural science (Holmwood and Stewart, 1991: 190, fn 23). Both are taken as the unchallengeable basis of investigation.

⁶ Although the mutual knowledge/common sense distinction is essentially methodological, it may be that some aspects of the former cannot be treated as the latter. This is the case when mutual knowledge cannot be formulated propositionally, as 'beliefs that some states of affairs or others are the case' (Giddens, 1984: 337).

grasped by the sociologist to characterise the practice. However, suggests Giddens, this does not preclude criticism. As Giddens states:

Mutual knowledge is the necessary medium of identifying what is going on when a sorcerer places a malicious spell upon an individual in order to procure that person's death. But this is no logical bar at all to critical inquiry into the empirical grounding that can be marshalled to support the validity of the belief-claims held in relation to this practice, or into their possible ideological ramifications. (Giddens, 1979: 252)

Thus seeing Zande beliefs as 'authentic' does not block critical social scientific inquiry into their validity. Nevertheless, social scientific accounts face two issues of adequacy (hermeneutical and empirical/critical) whereas natural scientific accounts only face one (empirical/critical).

I would suggest that Giddens' arguments for the dual character of social scientific investigations are problematic. To begin with, we have seen that the distinction between 'mutual knowledge' and 'common sense' is a methodological one. That is to say, it refers to two different ways of treating *the same stock of belief*. Holmwood and Stewart (1991) point out that Giddens contradicts himself over the treatment of this stock in social scientific investigation. As we saw above, when treated as 'common sense', a set of beliefs is argued to be 'corrigible' in the light of social scientific findings. However, this same set of beliefs, considered as mutual knowledge, was said to have been unquestionable for social science, that is, 'not corrigible in the light of its findings' (Giddens, 1984: 336). Thus, treating beliefs critically violates the conditions that Giddens sets up for hermeneutical understanding (Holmwood and Stewart, 1991: 32-7). Criticism undermines the 'mutuality' of belief, as it demonstrates a disagreement between lay actor and social scientist. It thus removes the uncritical acceptance of belief which Giddens argues is necessary to comprehend the social world. Critical social science must therefore involve a failure of understanding, as Giddens describes it. Conversely, the acceptance constitutive of understanding must block the development of criticism (Holmwood and Stewart, 1991: 34).

This raises the question of whether a set of beliefs that is problematic (i.e. subject to empirical criticism) can be accurately understood if it is treated as 'knowledge', that is, as fully adequate. I would argue that this cannot be the case, and such a treatment must necessarily *misrepresent* problematic belief, resulting in a failure of understanding on the part of the observer. If a set of beliefs can reasonably be criticised on some empirical basis then it must be demonstrably flawed on its own terms. According to the critical historicist approach introduced in Chapter 1, a flawed belief system includes residual categories which

do not cohere with those that systematically account for some phenomena. In other words, a belief system that can be reasonably criticised is one that contains internal incoherence. For Giddens, the 'hermeneutic' moment of investigation requires the social scientist to treat a set of beliefs as if it is fully adequate. This must involve presenting its categories as completely coherent, as any incoherence would demonstrate its inadequacy. However, if the set of beliefs is genuinely flawed, as its openness to criticism indicates, then presenting it as coherent must involve misrepresenting the relations between its categories⁷. Residual categories will be presented as if they fit systematically into the theory, obscuring their relations of tension and contradiction with positive categories. The result of applying Giddens' procedure would be misunderstanding, rather than understanding. An adequate comprehension of a flawed theory must involve grasping its flaws⁸.

One way to illustrate this is to consider the relation between a specific set of beliefs and social scientific criticism of them. Here we can consider beliefs discussed by Betty Friedan in *The Feminine Mystique* (1963). Friedan argues that throughout the late 1950s, American housewives suffered from the 'problem that has no name'. These housewives had different symptoms including depression, neuroses, listlessness and physical disorders such as fatigue and blistering (Friedan, 1963: 17-8, 28). The generality of the problem was slow to emerge, and Friedan argues that women's initial reaction was often to identify the cause as something specific to their lives, such as individual psychological problems, or particular issues to do with their husbands, children, houses or neighbourhoods. The social scientific criticism of these beliefs, put forward in accounts like Friedan's, focused on the generality of the experience across women, and postulated a structural explanation. This suggested that women's problems were not based in individual difficulties, but in the gendered division of labour which restricted their opportunities for self-fulfilment.

Let us now consider these lay beliefs and their social scientific criticism using Giddens' approach to social scientific adequacy. According to him, producing a social scientific account involves two phases, the hermeneutic and the critical. In the hermeneutic phase, the investigator suspends their scepticism, and actors' beliefs are treated as 'mutual knowledge', that is, as fully adequate. Applied to our example, this would mean treating the individualistic account provided by a housewife as a fully coherent account of the causes of

⁷ Giddens almost seems to recognise this when he comments that belief systems may produce logical contradictions on their own terms (1976: 152). However, this stray insight is lost in Giddens' formal statement of his approach.

⁸ Charles Taylor makes an argument to the same effect, claiming that the hermeneutic 'understanding' of actors does not require assuming that they are not confused or contradictory in their beliefs (Taylor, 1985: 24).

her problems. Then, suggests Giddens, we should treat beliefs as 'common sense', and empirically assess them. In our example, such an assessment would show the greater adequacy of a structural account like Friedan's in explaining the problems experienced by women.

This highlights the difficulties with Giddens' approach. If we accept that the social scientific critique of belief is valid, then we must also accept that the lay accounts were not adequate, and contained problems of coherence. We might, for instance, note that explaining neuroticism as an individual psychological quirk does not account for the prevalence of this condition among a particular group within society, that is, housewives in 1950s America. However, if the individualistic (lay) account indeed has flaws, then it cannot be correctly comprehended, hermeneutically speaking, by treating it as fully adequate. Such a treatment would misrepresent the relations between its categories, implying that they were fully coherent, rather than showing its inability to systematically explain certain evidence. In other words, Giddens' hermeneutical approach would require residual categories to be seen as positive, and incoherence to be rendered as coherence. This can only result in the mischaracterization of a set of beliefs. Surely, an adequate understanding of the lay account of housewives' problems requires a grasp of both its systematic elements and those things that it cannot account for. In other words, 'understanding' those beliefs means seeing their flaws as well as their advantages.

These criticisms are relevant to an assessment of Giddens' anti-naturalism. The separation of a hermeneutical and critical mode is precisely what is supposed to differentiate social scientific accounts from those of natural science. However, the above arguments suggest that such a separation cannot be defended. Instead of separate moments of 'uncritical understanding' and 'criticism', coming to understand a theory requires a comprehension of its flaws and incoherence which provides the grounds for criticism. I would argue that this is exactly the same process as can be found in natural science. Learning a particular scientific theory involves learning its positive and residual categories, where it is coherent, and where it is incoherent. Understanding Aristotle's theory of motion, for example, means understanding that he elides the notions of 'average speed over time' and 'speed at an instant', producing some incoherence in his accounts. The same approach must be taken to present-day science as well as superseded theories. Current theories are not 'understood' by viewing them as fully coherent; instead, scientists who 'understand' them grasp their problems, which provide the impetus for further development. Giddens' argument that mutual knowledge be seen as coherent and unproblematic thus gives it a status that would be

inappropriate even for the highly developed theories of natural science. To take a natural scientific theory as 'mutual knowledge' would be to suggest its finality and complete success in dealing with the world. In other words, it would be a form of epistemological realism. A better alternative is to see that whether we are attempting to grasp a natural scientific theory, or the beliefs of a social actor, understanding is not a matter of uncritical acceptance. In that all belief systems produced so far by human beings are in some way contradictory or problematic, such systems are misrepresented if they are characterised as perfectly coherent.

Some readers may agree that Giddens' treatment of social scientific adequacy is problematic, but argue that it nevertheless addresses an important concern which this chapter has not yet considered. In this light, Giddens' arguments could be seen as a flawed attempt to avoid 'ethnocentrism' on the part of the social scientific observer⁹. That is to say, if we argue that social scientists should take a critical attitude to the beliefs of their subjects, how are we to be sure that our judgements are not simply an imposition of our standards on their beliefs? Barnes and Bloor argue that cross-cultural judgements will always be ethnocentric in this sense, being based on the ultimately unjustifiable standards of the group making the judgement (Barnes and Bloor, 1982: 26-8). Their alternative, as we have seen, is to treat all sets of belief as adequate in their own terms, a move that parallels Giddens' hermeneutic phase of analysis (Holmwood and Stewart, 1991: 33).

The issue of cross-cultural judgement is a complex one, and is the subject of a vast theoretical literature, including important sociological contributions by Archer (1988), Lukes (1982), Hollis (1982) and Holmwood and Stewart (1991). There is no space to consider this literature here, but some brief points can be made drawing on the philosophy of science developed earlier and the work of Holmwood and Stewart (1991, especially Chpt. 3). Firstly, it should be noted that criticism of another set of cultural beliefs must be pressing in the terms of those beliefs. Criticism is oriented towards showing problems and difficulties that exist in belief, and a criticism cannot be valid if it claims as problematic features which raise no difficulties in the terms of the understandings being criticised. This could not be a valid criticism, because it would not be able to demonstrate its superiority. Instead, it would be a mere assertion of the importance of something which cannot be shown to be relevant in the terms of the other. This would be like criticising quantum mechanics for not being sociological. Such 'criticism' would have no purchase, and thus no validity, as it could not demonstrate why quantum mechanics would be enriched by taking on a sociological aspect.

⁹ I use this term broadly to refer to the prioritising of one's own set of cultural beliefs over those of another group, whether the differences map onto 'ethnic' distinctions or not.

The point is similar to that made about incommensurability in Chapter 1. There can be no genuine issue of comparison when theories are merely different to one another, and the inability to compare in such cases does not establish the existence of incommensurability. Likewise, the existence of different, non-competing theories does not demonstrate that criticism must therefore involve non-rational assertion of one over the other. Rather, it suggests that criticism does not apply in such cases, but exists where theories are disputing with one another, and one can show the problems with the other.

Those concerned to avoid ethnocentrism may still not be convinced by these arguments. It might be suggested that although a commitment to criticising beliefs in a way relevant to the terms of the other is a good thing, this commitment may not be enough. Particularly relevant here would be the concern that a critic might misidentify contradictions in the beliefs of others, making criticisms appear adequate when this is not the case. We can then ask: how can a critic test whether they have correctly grasped what is coherent and incoherent in the beliefs of another? Put simply, the answer is that an adequate grasp of the beliefs of others requires that one can successfully predict where their beliefs and actions will be successful, and where they will be unsuccessful and lead to frustrations and difficulties. To take an example from natural science, an Einsteinian physicist can say in which situations Newtonian theory will produce adequate predictions, and in which situations it will diverge from experimental results. Their ability to do so demonstrates a correct grasp of Newtonian theory and its problems on its own terms. Likewise in social science, we may be in a position to predict problems with a doctrine on its own terms. Referring back to the Friedan example, the social scientist can predict that the attempts of housewives to deal with neurosis through individual therapy would not remove the problem, as it would not increase their capacity for self-fulfilment. If a grasp of success and failure cannot be demonstrated, and the social scientist wrongly predicts where action will be successful and where it will fail, then their grasp of concepts is indeed inadequate. The important point is that the understanding of the social scientist is testable through such an exercise, and is thus subject to scrutiny. Of course, this does not mean that achieving such a grasp is straightforward, and it will frequently only emerge after lengthy and detailed study. But to refer to this as a practical necessity is not to suggest that there are insurmountable problems of cross-cultural criticism in principle.

If these arguments are correct, they cast doubt on the existence of one form of ethnocentrism feared by social scientists. Those who are concerned with the ethnocentrism of critical judgements frequently imply that a critical assessment can appear to be valid by

the standards of one group (the social scientists) even when it is not valid by the standards of another group (the subjects of investigation). If this situation could arise, the role of critical social science would certainly be in doubt, as its judgements would appear to involve the ethnocentric promotion of its own standards over others. However, I would argue that such a situation is not possible, and that *either* the critical judgement is not valid on social scientific terms *or* the beliefs of the subjects are not defensible on their own terms. This is because, as I have argued, for a social scientific account to be valid for the social scientists, it must correctly indicate problems experienced by the lay actors on their own terms. It does not make sense to suggest that a criticism is 'valid' for social scientists but not for actors. As Holmwood argues, if actors are not experiencing the problems that social scientists suggest must exist, this points to an error in social scientific accounts¹⁰ (Holmwood, 1996: 118-121).

2.3 Natural scientific knowledge and social scientific knowledge

In the previous section, I considered how Giddens uses the idea of a 'double hermeneutic' to argue for the differentiation of natural and social science, and paid particular attention to his claims that social scientific investigation involves special issues of adequacy. In this section, we shall consider further Giddens' use of the double hermeneutic, but this time in relation to other supposedly distinctive features of social scientific knowledge. The issues raised here are those relating to innovation in social science and the influence of social science on human conduct.

Giddens continues his argument for antinaturalism by distinguishing natural scientific knowledge and social scientific knowledge on the basis of their ability to innovate. For Giddens, innovation in the natural sciences is quite straightforward. He writes:

Theories in natural science are original, innovative and so on to the degree to which they place in question what either lay actors or professional scientists previously believed about the objects or events to which they refer. (Giddens, 1984: xxxiv)

Natural scientific theories can be 'revelatory' in that their resourcefulness reveals to us the errors of our existing beliefs about the object world. However, this is not the case in the social sciences. Giddens argues that social scientific theories are founded upon lay

¹⁰ Holmwood rejects the notion that competing accounts are each valid on their own terms, because it contributes to the protection of flawed sociological accounts (Holmwood, 1996: 118-121). He suggests that this notion is invoked when the superiority of a social scientific account to that offered by lay actors cannot be demonstrated due to the former's explanatory limitations. If each account is considered to be valid on its own terms, then the social scientific theory does not have to be reconstructed to improve its resourcefulness.

understandings, and because of this, they cannot be purely innovative in the way that natural scientific theories are. He states:

...theories in the social sciences have to be in some part based upon ideas which (although not necessarily discursively formulated by them) are already held by the agents to whom they refer. (Giddens, 1984: xxxiv).

In other words, the revelatory capacity is limited, because theories must be connected to the existing knowledge and meaningful conduct of actors. Establishing the characteristics of an already 'meaningful' world cannot produce surprises in the way that discovering new features of a meaningless object world can. Clearly, these arguments have connections with those concerning the adequacy of social scientific belief which were considered in the previous section. Giddens argues that a dual theory of adequacy is required in social science because social scientific theories must incorporate a non-innovative aspect, which is an account of the existing beliefs of agents. Having already discussed the question of adequacy, I would here like to critically reflect on Giddens' conception of innovation.

Once again, Giddens' distinction between natural and social science rests upon a differential treatment of issues of meaning in the two spheres. However, there is no tension between 'meaning' and 'innovation' in natural science, and once this is illustrated, the same arguments can be made in relation to social science. Recent work on the process of theory construction in natural science typically conceives of the production of new theories as involving the modification and development of existing conceptual resources (see for instance Kuhn, 1970). These resources may be existing theories about the specific domain being analysed, or can be drawn from other places such as different scientific fields or common sense. Once conceptual development has taken place, the resultant theory is scientifically innovative insofar as it supersedes the resourcefulness of existing theories in a field, and consistently accounts for phenomena that were problematic for earlier theories or absent from them altogether. Theories are meaningful, and natural scientific debates require the comparison of different meanings, but these meanings are considered as competitors, and innovation is the production of new, more successful meanings.

When natural scientific innovation is viewed in this way, it is hard to see why social scientific theory construction should be analysed differently. Certainly, it is plausible that social scientists will start their considerations from a critical understanding of existing activities within social life. However, this process is the same as natural scientists beginning from the existing theories in a field and developing them in order to improve their adequacy. Innovations in natural science result in theories that are more resourceful than those

previously available. Likewise, innovations in social science may involve the production of new theories which are more resourceful than 'existing theories of social life' that are embodied in lay activities. Just as the natural scientific investigator competes against other natural scientists (professional or lay), so the social scientist competes against other social scientists (professional or lay). In order to make comparisons, the meaning of lay actors' accounts must be grasped, but this does not restrict the possibility of innovation any more than the need to grasp the meaning of competing accounts in natural science restricts new development.

The apparent plausibility of Giddens' account actually rests upon an empiricist conception of natural science in which its innovations directly reveal features of the object world. When this is replaced with the notion that natural science attempts to produce more or less coherent theories (i.e. 'meanings'), our notion of innovation can be located within and between theories, and the production of more successful ways for us to deal with the world. The same point can then be made in relation to social science; it is oriented towards improving our dealings with the world, and producing better outcomes of human activity.

Having considered the question of innovation, we must now address Giddens' other arguments regarding the difference between natural and social scientific knowledge. These deal with the issues of transmission between 'meaningful' social scientific knowledge, and the 'meaningful' social world that it studies. For Giddens, the possibility of transmission between the two indicates a quite fundamental break between knowledge of the natural world and the social world. To see the nature of the comparison, we must start from Giddens' conception of natural scientific knowledge (Giddens, 1976: 79). Natural scientific knowledge is, at the present time, produced almost entirely within specialised professional institutions. This does not mean that concepts from natural science cannot filter down into lay discourse, and become understandings that are called upon in lay activities. However, this filtering process does not affect the laws of nature, which operate with the same character regardless of knowledge of them, professional or otherwise. Giddens argues that the same is not true of social scientific knowledge. This is because social scientific knowledge may be taken up by lay actors, and thus help to reconstitute the very domain which social scientists are studying. Giddens states:

...the appropriation of technical concepts and theories invented by social scientists can turn them into constituting elements of that very 'subject-matter' they were coined to characterize, and by that token *alter* the context of their application. (Giddens, 1976: 79. Author's emphasis)

Because social scientific knowledge is of the same meaningful substance as the constituents of the social world, its appropriation by actors may reshape the social world itself, and change the applicability of the knowledge. This has two main consequences. Firstly, it means that old theories may have a continuing relevance that is not found in the case of natural science. Giddens argues that an 'archaic natural scientific theory is of no particular interest once better ones have come along' (Giddens, 1984: 353). In social science, however, old theories are still important, and this is because they have shaped the social world which we wish to understand. Drawing on a specific example, Giddens writes:

Why, now that we are well familiar with the concept and the reality of state sovereignty, do seventeenth-century theories of the state retain a relevance to social or political reflection today? Surely exactly because they have contributed to constituting the social world we now live in. (Giddens, 1984: xxxiv-xxxv)

Thus, because of meaning transmission from scientific to lay discourse, old social scientific theories cannot be forgotten.

Interesting though this is, it is not the most startling of Giddens' claims. Rather he argues that the filtering of meaning from social scientific discourse to lay activities is one cause of the radical difference in status of laws of nature and laws of society. In the natural sciences, laws are held to be universally applicable. Giddens writes:

Universal laws state that whenever one set of conditions, specified in a definite way, is found, a second set of conditions will be found also where the first set causes the second. (Giddens, 1984: 344)

Laws are statements of causal mechanisms which operate regularly and have the force of necessity: one set of conditions necessarily brings about another.

According to Giddens, this is not the case in the social sciences. The reason for this difference is that the emergence of forms of knowledge, whether from social science or other activities, can lead to a reconfiguration of the social world, so that conjunctions can never be relied upon to continue, that is, to occur 'universally'. The reconstituting powers of knowledge mean that any generalisation about regularities in human conduct may be disrupted by actors learning about such regularities and altering their conduct accordingly. Giddens illustrates this point by discussing Machiavelli's writings. Machiavelli advises princes on how to gain the support of their subjects, offering insights such as:

When men receive favours from someone they expected to do them ill, they are under a great obligation to their benefactor; just so the people can in an instant become more amicably disposed towards the prince than if he had seized power by their favour. (Machiavelli, 1961: 69 quoted in Giddens, 1984: 350)

This appears to have the form of a universal conjunction. That is, when one set of conditions is fulfilled (the unexpected receipt of favours) another results (a populace better disposed to the ruler). However, argues Giddens, the knowledge of actors about Machiavelli and his writings may undermine such a conjunction. For example, if a ruler is thought by her/his subjects to be a student of Machiavelli, then actions such as the surprising distribution of favours may not be well received, being viewed as an intentional manipulation of public opinion (Giddens, 1984: 352). The conjunction between act and response is not a necessary one, but is dependent upon the knowledgeability of actors.

Giddens thus makes the bold claim that 'Laws in the social sciences are *historical* in character and in principle *mutable* in form' (Giddens, 1979: 243. Author's emphasis). Laws are historical because the conjunctions they refer to may only apply within a given period, until changes in actors' knowledge bring about new activities and new conjunctions (Giddens, 1984: 347). Because of this possibility, apparently law-like connections in social life are in fact subject to alteration. This provides a clear contrast with natural scientific laws, which are *universal* in their application and *immutable* insofar as human activity and knowledgeability cannot alter the laws of nature. This kind of argument is found in a number of writers, with the unpredictability of action due to developments in knowledge offered as an important consideration against naturalism (MacIntyre, 1985: 93-4) or a decisive one¹¹ (Taylor, 1985: 55-7).

As before, I would like to take issue with these antinaturalistic claims. This does not mean disputing all of Giddens' arguments, but suggesting that he looks for parallels between natural and social science in the wrong place. We should certainly accept that human activity does change with knowledgeability; it is this which gives the pursuit of knowledge centrality in human affairs. Giddens is thus right to say that we will not find universally applicable regularities of human conduct, if by that we mean invariant human responses to certain conditions¹² (as in the universalist construal of Machiavelli's principles). Furthermore, this has the potential to undermine certain naturalistic programmes within social science, such as that outlined by Jonathan Turner and Randall Collins in which the empirical characteristics of the social world are said to be generated from combinations of 40 law-like processes (Turner, 1992; Collins, 1992: 192-3). Turner defends his naturalistic position by rejecting Giddens' claims, and arguing that so-called mutable laws are in fact

¹¹ Bhaskar takes this same argument to be a demonstration of the necessarily incomplete nature of social science (Bhaskar, 1998: 48).

¹² Excepting, of course, those which are based in properties of the human organism, which Giddens recognises the existence of (Giddens, 1979: 244).

low-level empirical generalizations, and the exceptions to those generalizations which give the impression of mutability, can in fact be encompassed by higher-level, immutable theoretical laws. He further suggests that although agents may come to know these laws, the proof of their law-like character will be that no matter how much agents try to alter them, the fundamental social processes which these laws describe will still operate (Turner, 1992: 162). This latter argument is somewhat self-contradictory. Turner accepts that agents may learn about laws and thus attempt to change them. Even if we were to take these laws to be immutable, it would seem that the discovery of a law has altered the course of social life, as it has led to an agent performing an action (attempting to change a law) which would not have occurred otherwise. The production of knowledge has then reshaped human activity in a way which disrupts statements about the regularity of that activity. This point also undermines Turner's first argument, as no matter what level one retreats to, if the discovery of a 'law' alters human action in any way, then this suggests the ongoing evolution of human activity cannot be captured by universal law-like generalizations.

If Giddens is correct in these respects, then how can the homology of natural and social science be maintained? Let us first consider how to adequately characterise the natural scientific case. I have argued that natural scientific theorising can be understood as an attempt by human actors to find the most successful ways of dealing with the world through their theories. Certainly, regularity and the necessity of outcomes are important within these forms of investigation, and it is the universally consistent character of the results of theoretical and experimental activity that give investigation its worth. That is, in any specified situation, interacting with the world in a certain way will always produce the same results (setting aside purely random factors). If this was not the case, we would have no grounds for preferring one theory to another, as the coherency of a theory would be subject to inexplicable variations. Of course, this is not to deny the complexity of the world, with many factors feeding into actual outcomes. It is, however, to argue that this complexity is not inexplicable in character, but can be explained by analysing the regular elements which contribute to its construction. In this sense, the responses of the world to human activity are not 'mutable' and understandings are reworked and developed in order to better account for these consistent, necessary outcomes. Natural science can thus be characterised as a meaningfully constituted practice involving interactions with the world that produce regular effects. Scientific theories offer a more or less coherent understanding of these effects,

depending on their adequacy¹³. It is important to note, however, that it is not the *conduct* of natural scientists that is being analysed in terms of universal conjunctions. This conduct changes and evolves over time, as natural scientists produce new understandings, and alter their modes of interaction with the world. The regularity is not in their actual behaviour over time, but in the consequences that any specific mode of behaviour produces, and it is the latter which are formulated into laws.

This provides us with a model for understanding social activity. We should not envision laws of society which mechanically predict the behaviour of actors, because, as with natural scientists, this behaviour is altered when new understandings emerge. This does not mean, however, that we should term the limited generalisations of conduct which emerge 'historical mutable laws' and be satisfied with this, as Giddens suggests. Rather, we must insist that the crucial regularities lie in the *consequences* of employing social understandings. As in natural science, these are universally applicable, and action guided by the same understandings will always have the same consequences¹⁴, which may be more or less systematically understood by the actors involved. Using the terminology discussed in Chapter 1, if the consequences have a positive location in the system of understanding, the results of action conform to intention; if they occupy residual categories, the results of action contradict what was intended, causing problems for actors.

As with natural science, the task of social science is to improve our grasp of the necessary consequences of action, so that we can adjust our interactions with the world accordingly. In both arenas, statements of law are statements of the necessary consequences of some structure of activity. Marx's analysis of the 'capitalist mode of production' has this character, as it predicts the consequences of interacting with the world in a certain way, arguing that capitalism will necessarily lead to crises of overproduction and the immiseration of the proletariat. Despite the fact that they turned out to be empirically questionable, such claims have the correct form for laws, arguing for the necessary effects of a certain kind of interaction with the world. Such effects should be thought of as neither mutable nor historical (in Giddens' terms); so long as understandings of a certain kind are acted upon, particular consequences will necessarily follow, and their occurrence is neither historically bounded nor alterable by human beings. For example, whenever people engage in a structure of activity which conforms to Marx's analysis as capitalist, it will necessarily have

¹³ Remembering, of course, that these 'effects' are not 'factual', instead being theoretical constructions that are also reformulated in the process of inquiry.

¹⁴ Benton seems to make a similar point in relation to Bhaskar's version of the mutability argument, suggesting that whenever a certain social structure is instantiated its consequences are 'universal' (Benton, 1981: 18).

certain consequences. But this does not mean, as Turner's naturalism implies, that knowledge of a situation cannot change the course of human activities. Rather, the point is that human knowledge alters our activities so that we relate more successfully to the necessary outcomes of our interactions with the world. This is equally true in natural and social science.

If these arguments are correct, what bearing do they have on Giddens' earlier claims regarding the continuing relevance of old social scientific theories, and the relation between social scientific knowledge and lay activity? Certainly, he is correct to argue that social scientific knowledge may be incorporated into lay social action, leading to an alteration of actors' conduct. Such situations can be characterised as a process of learning whereby lay social activity (hopefully) becomes more successful through the incorporation of social scientific insights. This provides no warrant for antinaturalism, however, as the same process of learning occurs when an innovative natural scientific theory is produced, which results in the reconstruction of the activities of other natural scientists to (hopefully) gain from these insights. Natural scientists 'reconstruct' their theoretical worlds because of new understandings in the same way that social scientists and lay actors 'reconstruct' theirs. The filtering of meaning between scientific and lay actors has no special consequences for social science. It has straightforward parallels with the filtering of meaning between different natural scientific research groups.

It seems, then, that both social and natural scientific theories can be said to constitute the world for scientists, and to alter the theoretical worlds of others. The lack of interest in old natural scientific theories is not because they did not *at some time* act as constitutive elements of scientists' worlds. The important point is that, due to the impressive rate of advance in many fields of natural science, these worlds have been *reconstituted* by newer theories which have greater coherence and consistency. Old theories are uninteresting because they are less resourceful than our present-day theories. Thus, the issue in relation to social science cannot simply be that its theories can reconstitute society. What Giddens is implying is that old and superseded theories are still relevant in the social sciences, unlike the natural sciences, because they are still constitutive of the social world. There are two possible responses to this claim. Firstly, one could argue that the failure of new theories to reconstitute the social world actually suggests that they are not more resourceful than those they were intended to supersede. The persistence of old understandings would be evidence that professional and lay social scientists have not yet come up with better ones. Secondly, it could be argued that better understandings have been produced, but that the institutional

structure of a society may not allow for the transmission of successful ideas and their practical incorporation into human activity. Such a situation would have to be addressed by investigating these social structures and how they themselves could be reconstructed. I do not myself know which of these alternatives is correct. What should be noted is that both raise issues of success in social life in the same way as these emerge in natural science, contrary to Giddens' arguments.

2.4 Agency and social science

This chapter has already discussed a range of arguments that Giddens uses to differentiate social and natural science. However, one more set of claims must be addressed before this survey of Giddens' arguments can be complete. These relate to issues of agency that are relevant in the social world, but have no correlate in the natural realm. According to Giddens, social science has to accept the existence of agency in order to properly understand the character of social activity. Agency is defined by Giddens as '*the stream of actual or contemplated causal interventions of corporeal beings in the ongoing process of events-in-the-world*' (Giddens, 1976: 75. Author's emphasis). A further crucial point is that agency is not merely a matter of causal influence but necessarily involves the notion that an agent 'could have acted otherwise' (Giddens, 1976: 75). That is to say, an agent always has some degree of choice about what course of action to follow, and is never simply compelled to act. This lack of compulsion is intimately bound up with the meaningful nature of human conduct. Action is not simply an automatic or mechanical response to some situation or set of conditions, but involves a meaningful assessment of this situation, and a decision to act in one way or another based upon the wants of the actor. Giddens terms this capacity 'agent causality', which he discusses in *New Rules of Sociological Method*, stating:

That action is caused by an agent's reflexive monitoring of his intentions in relation both to his wants and his appreciation of the demands of the 'outer' world supplies a sufficient explication of freedom of conduct for the needs of this study. (Giddens, 1976: 84-5)

Because actors can reflect about possible actions on the basis of their wants, they are not compelled to follow any course of activity.

Giddens does acknowledge that there are situations in social life when actors appear to be forced to act in one way rather than another. However, he suggests that situations which seem to involve compulsion rather than choice only appear to do so when we leave out the role that meaning has to play. For example, we typically think that when an agent is threatened with death unless they follow a specific course of action, this is a matter of

straightforward compulsion. However, suggests Giddens, this is not the case, as the threat of death only carries weight if the individual values life, and the situation is thus mediated by human meaning. Giddens argues that the example can be reformulated as follows:

To say that an individual 'had no choice but to act in such and such a way', in a situation of this sort evidently means 'Given his/her desire not to die, the only alternative open was to act in the way he or she did. (Giddens, 1984: 175)

In other words, the threat of death does not simply compel an agent by itself; it must be mediated through the agent's understandings and desires, and it only has an influence because of these¹⁵.

As is typical of Giddens' approach, the idea that forces and constraints are mediated in the human realm is contrasted with the 'implacable' forces that are found in nature. When Giddens discusses social structural constraints he writes that:

They [structural constraints] cannot be compared with the effect of, say, an earthquake which destroys a town and its inhabitants without their in any way being able to do anything about it...The structural properties of social systems do not act, or 'act on', anyone like forces of nature 'compel' him or her to behave in any particular way. (Giddens, 1984: 181)

The natural realm involves mechanical compulsion, and the social realm does not. Thus, social science must acknowledge the possibility of agency in its analyses.

The problems with Giddens' arguments here are similar in character to those discussed in the previous section. Giddens contrasts natural and social science on the basis that social science must acknowledge the mediation of causal forces by 'meaning'. However, if we consider both natural science and social life to be meaningful attempts to deal successfully with the causal impact of the world, then issues of compulsion take on a different hue. After all, even though the processes studied by natural science are forceful and mechanical in character, no-one would argue that by that token natural scientists are simply impelled to theorise them in one particular way. That is to say the natural world does not *make* theorists categorise it in one manner rather than another. Clearly, there are aspects of the natural world which have been theorised in different ways throughout human history, as evinced by the competing astronomical theories of Ptolemy and Copernicus. Furthermore, no mechanical force would stop human beings from giving up present-day theories, and reverting to earlier ones in their interactions with the world. Nevertheless, our sense that this would be an irrational decision indicates that the mediated and meaningful character of

¹⁵ Archer makes an analogous claim, suggesting that where constraints appear to be working in a 'hydraulic' manner, this is because we take the desires of the agents involved for granted, for example the desire not to starve (Archer, 1995: 199).

natural scientific theories does not make commitment to them optional. As argued in Chapter 1, a theory is pursued because it systematically accounts for the impact of the material world, and to revert to a theory that is less resourceful would be to opt to find the world a more confusing place. Although our interactions with the natural world are 'meaningful', it is wrong to say that the understandings we employ are thus a matter of choice. Rather, we decide between theories by assessing how systematically they allow us to deal with the causal forces and influences that we encounter. To 'choose' to employ an unsuccessful theory would be irrational.

When natural scientific investigation is viewed in this way, the activities of social agents seem to have a similar character. Giddens suggests that agents make decisions based on an assessment of their situation oriented towards meeting their particular wants. However, I would argue that this does not introduce an element of 'choice' into matters. If, for the moment, we take actors' wants to be fixed, then the compelling course of action to a rational actor is that which comes closest to fulfilling those wants. A lack of choice is a feature of the *rationality* of the situation, in that if actors want to achieve certain goals, then they must act in accordance with the best available knowledge about how to do so. If, for example, an actor values their life most highly of all, then it does not make sense to say that they have a choice when presented with only two options: death or another course of action. They are constrained to pursue the alternative course as the best available option for realising their wants. As Holmwood and Stewart argue, to 'choose' against one's wants would be a denial of self, that is the priorities which constitute one as a self¹⁶ (Holmwood and Stewart, 1991, Chpt. 6). This does not mean that deciding to act in a certain way involves strict physical compulsion, but the same is true in deciding between theories in natural science. In both cases, meaningful reflection on alternatives is rationally compelling in that it locates the most resourceful known option.

Is an element of choice introduced if we argue that wants may themselves be reflected upon? It is hard to imagine that human beings could alter their wants with complete freedom, given our biological basis and the construction of somewhat stable selves through various psychological and sociological processes. Nevertheless, we do seem to be able to reflect upon our wants, and reprioritise them, so that goals that were important at one time later become subsidiary or irrelevant. It seems to me, however, that such reflection on priorities cannot be seen as a matter of 'choice'. If reasons are found to reprioritise wants

¹⁶ If such a 'choice' was taken, the analyst would have to reconsider their conception of the actor's wants, given that these are defined as the priorities which actors wish to pursue.

then, as with the other questions of knowledge considered, these cannot be taken as 'optional', but are compelling insofar as they demonstrate why the changed order of priorities is better for the actor. A person may decide that some element of self is repressed in their current order of priorities and alter these as such. For example someone may realise that their love of the outdoors is more important than their desire to understand the social world, and thus resign their job as a sociology lecturer to become a lumberjack. Insofar as the decision is a reasoned one, it does not involve a 'choice', but an apprehension of the best way to achieve what one thinks is valuable, and a pursuit of this option¹⁷.

Thus, in the reflection on and pursuit of wants, issues of 'choice' do not have a central, foundational position. Giddens argues that choice is central because he believes that the meaningful character of social activity gives human beings a distance from the demands of the 'outer world'. However, the existence of meaning does not give human beings a 'choice' in relating to this material environment, as the meanings employed are more or less adequate, allowing more or less resourceful activity. Given that activity is bound by what Holmwood and Stewart call the 'constraints of competence', a choice to act against these constraints would be the embrace of incompetence (Holmwood and Stewart, 1991: 100-1). Although Giddens would surely reject this as an inadequate conception of agency, he does not show how it can be given a more positive characterisation.

These arguments cast doubt on the notion that there is a special issue of agency in social life. The issues of meaning invoked to defend such a notion are, in fact, the same across natural scientific investigation and social activity more generally. To illustrate this, we can return to Giddens' claim that social constraints, such as the threat of death, are not like natural forces, such as an earthquake which destroys a town. These cases are slightly disanalogous, and if this is rectified we can see that there is no issue of agency in the first case which differentiates the social from the natural. A better parallel to the threat of death in social life is an earthquake warning given to inhabitants that they are in serious danger if

¹⁷ Of course, it could be the case that knowledgeable reflection using some set of criteria produces two equally viable options. There could, for example, be two ways to achieve the end of 'spending more time in the outdoors' which were equally promising using all of the criteria of importance to an actor. In such cases, it is reasonable to speak of a 'choice' in relation to action, but if decision-making criteria are truly exhausted then this choice must be a random one. This does not support Giddens' general contention that 'choice' is analytic to agency, as it exists only in a subcategory of cases, i.e. where there are equi-rational options. Furthermore, this kind of choice has a direct correlate in natural science where two theories come out as equally valid on the relevant set of criteria.

they do not evacuate from a town¹⁸. As Giddens points out, the threat of death is irrelevant if one does not value life. Equally, however, the possibility of dying in an earthquake is irrelevant to those who do not value their lives. In both cases, human responses are governed by a knowledgeable assessment of the best course of action. Given our wants and our knowledge of possible actions, we have no reasonable option but to follow that course which seems best for us.

2.5 Conclusion

In this conclusion I would like to emphasise the relevance of the arguments made here to the overall themes of the thesis. The contention of this thesis is that natural scientific investigation and other social practices have the same general characteristics. They are activities oriented to a successful interaction with the world, whose success is not based in some unshakeable foundation, but is instead a product of the resolution of problems of understanding over a period of time. By contrast, Giddens' anti-naturalistic approach divides the characteristics of natural science from those of other social activities. The implication of his arguments is that because social activities are constituted by 'meaning', issues of their success are different to those of natural scientific theories. I have argued throughout the chapter that this distinction cannot be sustained. Giddens claims that actors' understandings must initially be treated as perfectly successful 'mutual knowledge' before they can be critically assessed. This differentiates them from the understandings of natural science whose success can be straightforwardly assessed through empirical means. I suggested that this treatment of actors' understandings must misrepresent them, and that, as with natural science, comprehending the beliefs of actors involves understanding the problems with them. The idea that social life involves agency also divides natural scientific investigation from other social activities on the basis of success. Giddens' argument is that because social life is meaningfully constituted, social actors have a 'freedom to do otherwise' that is not present in human interactions with the natural world. The suggestion is that the latter are bound by the consequences of action which will be more or less successful in relation to the material world, whereas social action can escape these constraints. However, I argued that Giddens fails to show that success is not binding in social life. Its meaningful nature does not mean that actors have a 'choice' in their actions. As with natural

¹⁸ It would be wrong to argue that the option in Giddens' social example is always present in social life and never present in human relations with the natural world. An actor may simply be shot with no offer of an alternative course being given; likewise, we may be forewarned of events in the natural world allowing us a chance to deliberate upon responses.

science, the meanings involved are more or less successful in producing outcomes for actors, and any 'choice' to go against what seems to be the best option must be suboptimal for actors. Giddens' attempts to distinguish social practices from natural science lead him to give an incoherent account of the former.

Giddens' approach also generates problems in his treatment of natural science, which is, at the very least, ambiguous. He certainly acknowledges the importance of post-positivist arguments that scientific investigation is constituted by 'frames of meaning'. He also accepts that natural science is a part of social life (see for example, Giddens, 1976: 79). However, Giddens wants to contrast the social and natural worlds, and in doing so, the place of natural science in his analysis becomes unclear. As natural science is related to both the social and natural worlds, Giddens must hold two contradictory postulates about it. On the one hand, because science is a social practice, its world must be produced by the skilled and knowledgeable activities of agents. On the other hand, because science is oriented to non-social nature, its world must be that of external objects which exist independently of meaning. To avoid this contradiction, Giddens underplays the meaningful (that is, theoretical) character of science, and implies that it relates directly to the world of objects. However, this leaves him with an unsatisfactory, foundationalist view of science. One example of this is Giddens' account of the differences between natural scientific and social scientific innovation. As we saw above, he argues that innovation in social science is distinct to that in natural science because the social world is meaningfully constituted. In order for this to be a genuine contrast, the inference must be that innovation in natural science is not meaningful (that is, theoretically mediated) but is instead directly revealing of features of the object world. It was argued in Chapter 1 that such a position is untenable, and that even the most apparently 'direct' concepts of experience are theoretical mediations that may be reconstructed in the process of investigation.

The move towards a foundationalist account of science is a result of Giddens' anti-naturalism. His claim that social life has issues of meaning not present in natural science pushes analysis of the latter towards the idea that natural science involves 'direct' apprehension, rather than theoretical mediation. I would argue that it is more satisfactory to avoid this division, and accept that both natural science and other social practices are meaningful in character, and variable in their success in dealing with the world.

3

Sociology as Science?

Realism and the Question of Agency

3.1 Introduction

Realist¹ philosophy emerged in the 1960s and 1970s as a non-positivist account of natural science, and has developed, in the British context, into the leading philosophy of social science that continues to draw inspiration from the achievements of natural scientific inquiry. As a contributor to the demise of positivism, realism distinguished itself from other critical frameworks by placing a strong emphasis upon the 'reality' of material structures within the natural world. Realists claim that natural science is possible only because of the existence of such structures independent of human knowledge of them. They argue that the task of natural science is to investigate the properties of real structures and make (fallible) attempts to know their characteristics.

As a philosophy of social science, realism argues that social life contains structures which can be known scientifically. In this respect, it claims to be naturalistic in orientation, paralleling natural scientific and social scientific inquiry. Realists often contrast their 'scientific' approach to social life with professed antinaturalisms, such as that of Giddens. From a realist perspective, Giddens' emphasis on the 'meaningful' constitution of social life undermines a rigorous consideration of the objective structures of society, which realists claim exist independently of subjective human apprehension of them (see for example Archer, 1995, Chpt. 4 especially).

Nevertheless, it is the argument of this chapter that realism does not avoid the division of science and society, and the problems that this entails. Although realism emphasises the connection between natural and social science, its proponents argue that social life has special features not present in natural scientific investigation. Roy Bhaskar, one of the founders of the realist approach, claims that social life involves the activity of agents, in addition to the operation of structures. Social structures are amenable to scientific analysis,

¹ I used the term 'ontological realism' for this position in Chapter 1 to distinguish it from epistemological realism. In this chapter, unless the term is used with a modifier, it will refer to ontological realism.

but human agency adds another dimension which is not present in natural science (Bhaskar, 1998: 37-44). I shall be arguing that this division of science and society is as problematic for realists as it is for anti-naturalists such as Giddens. On the one hand, realists cannot give positive substance to the non-structural features of society bracketed under the notion of agency. On the other hand, the conception of structural analysis offered by realism does not successfully capture the historical and theoretically mediated character of scientific knowledge.

To discuss these claims, I will be focusing upon the arguments of Margaret Archer. Archer is an important exponent of the realist cause, and her *Realist Social Theory: The Morphogenetic Approach* (1995) has been the subject of much recent discussion (see for instance Healy, 1998; King, 1999; Zeuner, 1999). Realism is not a homogeneous perspective, but it is fair to say that Archer's work is a development of the mainstream realist approach that draws on the work of Roy Bhaskar (1978, 1979). In this respect, it is consonant with important realist works such as Andrew Collier's *Critical Realism* (1994) and William Outhwaite's *New Philosophies of Social Science* (1987). A critical appraisal of the work of Archer thus addresses the dominant perspective within the British realist tradition.

The argument of the chapter proceeds as follows. Firstly, I outline Archer's morphogenetic approach and its dualistic attempt to analyse structure/agency, culture/agency and structure/culture relations by separating each element out and then theorising their interplay. The main body of the chapter is then devoted to a detailed consideration of Archer's arguments and the problems that her separation of structure, culture and agency generate. The concluding section reconnects these matters with the themes of the thesis.

3.2 Morphogenesis and realism

Archer's work is intended to offer a coherent framework for analysing the development of cultural and social structures, and the processes which contribute to their stability or change. Although the initial inspiration for her work was systems theory, Archer discovered the connections between her morphogenetic² approach and the realist perspective, arguing that the former is a sociologically rich version of the latter (Archer, 1995). The important tenet

² Archer draws the term 'morphogenesis' from Walter Buckley's work (Archer, 1995: 137) and it refers to her theory of the causes of stability or change in systems. There is some potential for confusion here in that the term is also used to refer to one of two possible outcomes of a system configuration, that is morphogenesis (a change in the system) as opposed to morphostasis (no change in the system).

which the two share is that the objective structures of a social system must be theoretically separated from the agency of the actors involved in it (Archer, 1995; Bhaskar, 1998). Archer thus subscribes to an 'analytical dualism' which separates out the influence of the 'people' (agents) from that of the 'parts' (structures) and analyses their interrelation over time. In the terms of this thesis, Archer divides science and society, arguing that society contains not only scientifically analysable structures, but also human agents who may act independently of these structures. To theorise this, Archer draws on David Lockwood's article 'Social integration and system integration' (1992 [1964]) in which he argues that by separating the characteristics of social groups and social structures the analyst can better account for social stability and change. Lockwood's key claim is that although there may be tensions in the relations of institutional structures to one another, this may not produce change if these tensions can be suppressed by social groups with a particular interest in doing so (Lockwood, 1992: 407-8). Archer expands on Lockwood's arguments to offer a dualistic mode of analysis that applies to both social and cultural systems, which she then draws together into a wider, more general framework.

To grasp the character of Archer's system we need to examine how she theorises the separation of levels in her analysis. Her analytical dualism is intended to distinguish two aspects of society that have distinct properties. On the one hand there is the 'system' level, which contains relations whose objective character is independent of the knowledge of any actor. This sets up an objective 'situational logic' which agents have to deal with. However, this alone is not enough to understand the character of society and societal change, because actors are not merely 'bearers' of the objective properties of the system (Archer, 1988: xiii). Rather, how actors deal with these objective properties is an independent question, and one which relates to the characteristics of the 'social' level in which the properties of groups of agents and their strategic activities come to the fore.

In the cultural sphere, the 'system' consists of items of culture that can be expressed propositionally and the logical relations between these, most importantly their relations of contradiction or coherence³. These are combined with the 'necessity' or 'contingency' of the relation between beliefs⁴ to make four possible combinations. One example is the 'constraining contradiction' in which belief A necessarily evokes belief B which contradicts it (Archer, 1988: 148-9). Another example is the 'contingent complementarity' in which two

³ Archer adds that items could also be logically independent of one another, but argues that this is unimportant compared to relations of coherence or contradiction (Archer, 1988: 105).

⁴ I have referred to relations between 'beliefs' here, but Archer states that the items being compared may also include systems, aspects within a system, or a subsystem and its 'ideational environment' (Archer, 1988: 147).

items within the Cultural System are coherent with one another, but neither ineluctably invokes the other (Archer, 1988: 219). Archer argues that propositions and their relations exist in what Popper (1972) calls 'World Three', which is the realm of objective contents of thought. 'World Three' relations can be distinguished from the subjective beliefs of individuals. As Archer puts it:

That the ideas of, say, Buddha agree with those of, say, Schopenhauer is to say nothing about the subjective mental experiences of the two people - it is a *logical* statement... (Archer, 1988: 105. Author's emphasis)

This 'objective' character is not only independent of subjective individual judgement, but is also, according to Archer, cross-cultural. She disputes the claim put forward by constructionists such as Barnes and Bloor that logic is culturally relative, arguing that conceptions of logic in fact show a great regularity across history and culture, and supposed alternatives are either minor variations or misconstruals on the part of the analyst (Archer, 1988: 112-127). Therefore, the relations within the Cultural System of any society can be objectively characterised as having certain properties of contradiction or coherence.

Within the cultural sphere, Archer refers to the level pertaining to the properties and strategies of groups as the 'Socio-Cultural'. Whereas relations within the Cultural System are logical, those within the Socio-Cultural level are causal connections between groups and individuals. These relationships are based in material and ideal⁵ interests, which provide motivation for groups to uphold or challenge certain sets of beliefs (Archer, 1988: xx). Actors take up beliefs to achieve the 'articulation, assertion, or legitimation' of their interests, and a coherent defence of beliefs furthers these (Archer, 1995: 306; Archer, 1988: 235-42). This is also the level within which power operates to influence the uptake of beliefs by different actors. So, for example, a group that is socially powerful may 'use their power to control the visibility of inconsistent items' in their own doctrines, limiting access to the ideas through means such as censorship (1988: xxi).

⁵ What exactly constitutes an 'ideal interest' is not defined by Archer. Its meaning is not explicitly specified in her work, and the main clue in *Culture and Agency* is a brief list of exemplars given of such interests which are said to include 'ethnic, religious or linguistic divides' (Archer, 1988: xx). In *Realist Social Theory*, Archer suggests that there are objective but non-material benefits from continuing to hold to the beliefs of one's community, including status, position, friendship and support among members (Archer, 1995: 212). She calls these 'cultural benefits' but it seems plausible to see these as the substance of an 'ideal interest'. If this is the case, then the notion of 'ideal interests' is of poor explanatory value. It does not explain why the community holds *these* beliefs rather than any others, something which Archer's materialist analysis at least purports to account for. This could perhaps be attributed to chance. However, such a move then raises the question: why would a community reward the defence of a set of beliefs that they have no material motivation to defend, and which they came upon by chance?

Having separated out these levels and their characteristics, Archer's theory of their interrelationship is termed the 'morphogenetic cycle'. This cycle is a method of analysis which 'breaks in' to the flow of activity for the purpose of theorising the change or stability of cultural (and social) structures due to the interplay between the levels (Archer, 1988: 106, 144). The cycle begins with the Cultural System in a certain state, its constituent propositions having objective relations of coherence or contradiction to one another. This is a 'situational logic' which exerts causal influences upon actors at the Socio-Cultural level, conditioning their possibilities for action (Archer, 1988: 143). When actors hold beliefs that 'stand in manifest logical contradiction or complementarity to others', this places a causal constraint upon action (Archer, 1988: 145). One example of this is when actors find themselves in the situational logic of constraining contradiction (1988: 148-9). In this case actors wish to maintain a belief, A, for reasons that reside on the Socio-Cultural level. However, belief A necessarily evokes belief B, which is in contradiction with A. This, Archer argues, places the actors under strong constraints, because they cannot reject belief B (due to its internal connection with A), but on the other hand, the contradiction involved threatens to make A appear indefensible. So long as they wish to hold A, actors have a logical constraint to deal with.

The strategies actually pursued by actors are the subject of the next phase of the morphogenetic cycle, Socio-Cultural interaction. Although interaction is structured by the properties of the Cultural System, these do not determine it. Rather Socio-Cultural action is pursued on the basis of the interests of social groups, and the existence of human agency means that this action may be original and creative (Archer, 1988: 187). Actors respond to the logic of the cultural system and attempt to make their responses Socio-Culturally efficacious. This in turn influences whether the outcome of action is system change or system stability. To see this in operation, we can consider the aforementioned 'constraining contradiction'. As we have seen, this places actors who have interests in defending belief A in an awkward position. However, although the Cultural System is contradictory in this respect, the Socio-Cultural level may not be, and the group wishing to defend A may wield a great deal of power and influence. If this is the case, suggests Archer, they may be able (for a while) to conceal the contradiction from the general public and avoid the charge of inconsistency (Archer, 1988: 189-197). The result of this is morphostasis, at least temporarily, and a protection of the interests of the dominant group (Archer, 1988: 188, 226).

This brings us to the final stage of the morphogenetic cycle, cultural elaboration. Depending on the characteristics of the Cultural System, and Socio-Cultural response to them, the system may either remain broadly stable or change in its characteristics. As we have seen, the combination of constraining contradiction and a strong interest group concerned to conceal it produces morphostasis in the short term, with no substantial elaboration. However, other configurations lead to the production of new cultural items. For example, competing Socio-Cultural groups may each choose to defend a different theory in a situation where the theories are contingently contradictory to one another. In such a situation, each is pushed to criticise the other's theory, and also to elaborate their own in defence against criticism. The result is the production of new items in the Cultural System which are strongly differentiated from one another due to the competition of interests that produced them (Archer, 1988: 246). More generally, whether the Cultural System is elaborated or remains the same, the objective relations between its elements feed back in as the first phase of the next morphogenetic cycle, providing a situational logic that actors must once again respond to.

Archer uses exactly the same form of analysis to theorise social structure, separating out two aspects of society and then theorising their interrelationship. In analysing the System level, Archer calls upon realism to argue that society contains objective structures which have the character of necessity and materiality. Explicating the first of these, Archer argues that structures are not made up of contingent combinations of elements which could dissolve at any moment, but have a causal influence through their intrinsic nature, as do structures in the natural world (Archer, 1995: 173; Bhaskar, 1975). Turning to the second aspect, the 'materiality' of structures is what distinguishes them from the objective elements of the Cultural System, ideas. Structures have characteristics that are not based in ideas, and they typically have causal influence independent of what is believed about them (Archer, 1995: 175). As a consequence, the meanings that can be 'imposed' on structures are limited by their material nature (Archer, 1995: 176). By way of illustration, Archer refers to structural factors such as famines, demographic structures, and income distributions which have an 'objective influence' independent of the interpretations of 'readers', and have an effect on actors regardless of whether they comprehend it or not (Archer, 1995: 112).

Given that structure can have objective characteristics, we need to consider how the configuration of the structural level impacts upon the possibilities for actors⁶. The most

⁶ I have excluded discussion of Archer's notion of roles here, as it does not play a central part in her analysis, and is not relevant to the criticisms that I will later offer.

important feature of structures is that positions within them offer differential access to valued resources that are socially scarce. Positions are defined by the relative advantage or disadvantage they give to actors holding them, that is, the relative access to resources that they offer (Archer, 1995: 204). The benefits or otherwise of a position give its holder a vested interest in either the maintenance of current structures or their reconfiguration. Those who hold advantageous positions in the current structure have an interest in its maintenance; those whose positions give them disadvantages have an interest in its alteration (Archer, 1995: 203). This can be paralleled with Archer's analysis of the Cultural System in which social groups have an interest in either upholding some belief (interest in maintenance) or challenging it (interest in change).

We have seen so far that actors derive relative advantage or disadvantage from particular institutional structures. Now we have to consider the influence of structural relations on this, that is, relations of strain or compatibility between institutions (paralleling the logical relations between cultural items). There are four types of objective relations between structural elements, depending on their compatibility or incompatibility and the necessity or contingency of their connection (Archer, 1995: 218-229). Each combination produces a different 'situational logic' for actors, encouraging them to respond to the institutional configuration in different ways. One example of this is the 'necessary complementarity', in which institutions are necessarily related to one another, and their operation is compatible. To illustrate this, Archer refers to the internal and coherent connections between 'ancient Indian institutions' such as caste, religion, kinship, polity and law (Archer, 1995: 219). She argues that this compatibility makes it objectively rational for all actors to support the institutions because 'everyone has something to lose from disruption' (Archer, 1995: 220). Even those who are lower down in the caste hierarchy have local control over certain material benefits which would be lost if actors attempted to disrupt the institutionalised set-up (Archer, 1995: 220-1). The situational logic, then, is one of protection of the institutional order. Of course, this varies with different institutional configurations and a situation in which structures are contingently incompatible, for example, encourages a situational logic of elimination whereby the supporters of each institution have something to gain by eliminating the other (Archer, 1995: 225-6).

It should be emphasised again that, in a realist spirit, these are all 'objective' properties of institutions that have a certain character regardless of how actors interpret them. To move to the next phase of the morphogenetic cycle, from structural conditioning to interaction, we need to consider the properties of agents. Archer makes a strict distinction between structure

and agency, that is, between the characteristics of the parts and the people. In doing so, she ties her work in with that of Bhaskar, who argues for the separation of structure and agency in *The Possibility of Naturalism* (1998 [1979])⁷. Archer's treatment of agency in relation to structure is more complex than her analysis of the culture/agency relation. The two key aspects of it are the activities of individual persons and those of social groups⁸. As individuals, actors face an objective structural logic. However, because humans are reflexive and have the ability to assess the situations in which they find themselves, agency must be taken into account (Archer, 1995: 249-250). Agents are capable of an altruistic renunciation of their interests, and acting in relation to moral concerns and not material ones (Archer, 1995: 206-213). Even though individuals are faced with objective structural reasons to act in a certain way, it is still up to them to weigh these reasons and find them good (Archer, 1995: 209). The outcome of this process is not predetermined.

The other central aspect of agency pertains to the activity of collectivities. A 'collectivity' is defined by Archer as a group of persons who, because of some structural influence, share the same life chances (Archer, 1995: 257). There are two types of collectivities in Archer's analysis, Primary and Corporate Agents. Primary Agents are those collectivities where people share life chances but do not express their interests as a group or 'organize for their strategic pursuit' (Archer, 1995: 259). Corporate Agents are collectivities in which the members have organised into an interest group that self-consciously pursues their own advantage (Archer, 1995: 258). In relation to agency, Corporate Agents are able to strongly influence whether a system stays stable or changes, because of their organisational power (Archer, 1995: 258). In contrast, Primary Agents may have an influence through the aggregate actions of their members, but this is uncoordinated and unfocused in character. (Archer, 1995: 259). Archer's point here is that it is not enough to know the structural logic of some situation. Rather, one also needs to know the configuration of agents and their powers to grasp what system state is likely to ensue.

This brings us to the final aspect of structural analysis, the elaboration of structures. Whether structural morphogenesis or morphostasis occurs depends on a combination of the properties of structure and of agency. A specific structural logic of advantage/disadvantage gives existing agents reasons to either support the existing structure or try to alter it. However, whether change occurs or not depends on the strength of the agents involved

⁷ Bhaskar writes: 'I want to distinguish sharply, then, between the genesis of human actions, lying in the reasons, intentions and plans of people, on the one hand, and the structures governing the reproduction and transformation of social activities, on the other...' (Bhaskar, 1998: 35).

⁸ Once again, I am leaving out the element of agency associated with social roles, which, although relevant to Archer's analysis is not analysed in detail, and is not relevant to my treatment here.

(Archer, 1995: 297-302). This strength derives from the resources available to them as a collectivity, their successful mobilization of these, and their position in relation to other groups. So, for example, the institutional logic of 'necessary contradiction' will give some Corporate Agent an interest in containing the contradiction as long as possible, to retain their relative advantage (Archer, 1995: 303). However, their ability to do so depends on the 'relational negotiating strength' between the Corporate Agent promoting the state in question and the others who might oppose it. In this case, if the agent is successful, then morphostasis is the result. On the other hand, structural change may result in which groups may reform and resources may be redistributed.

A final, brief, mention should be given to the interrelation between structural and cultural levels, which completes Archer's Byzantine theoretical system. So far, we have considered Archer's analysis of each level separately. However, Archer argues that the two interpenetrate one another. In order to theorise the result of such interconnection, Archer analyses the four possible combinations of system logic, which depend on the morphostasis or morphogenesis of each system (Archer, 1995:308-324). When the two systems line up, as, for example, when both are going through morphogenesis, then the process is intensified in each system. On the other hand, when one system is going through morphostasis and the other morphogenesis, then the stability of the one exerts a drag on change in the other.

3.3 Structure, agency and interests

As we have seen, when analysing the structure and agency relation, Archer insists on two main points: (i) the objectivity of structure, which, due to its material character, has a causal influence independent of agents' understandings; and (ii) the capacity of agents to be reflexive and creative, taking a strategic attitude to structural possibilities. In this section I want to argue that Archer has trouble defending both of these claims. The general difficulty is that those activities attributed to agency either become sociologically inexplicable, or appear to involve the suboptimal behaviour of actors in relation to structural possibility, rather than demonstrating actors' reflexivity and creativity. Once society is divided from science, it is analytically difficult or impossible to give social agency a positive statement. As science is held to identify the most resourceful possibilities for action, any divergence from scientifically identified structures must be problematic for agents.

This difficulty first emerges in Archer's account of the relation between structure and the agency of individual persons. Archer argues that individuals inevitably act in a context that is socially structured, and structures condition action without the 'compliance, consent or

complicity' of actors (Archer, 1995: 200). Depending on their structural location, actors find themselves with relative advantage or disadvantage in their access to material and cultural resources. This gives them objective opportunities that they can take up to pursue their projects, and if these options are eschewed, actors pay an opportunity cost (Archer, 1995: 205). It also gives actors a 'vested interest' in either structural stability or change, depending on the benefits of their position (Archer, 1995: 205).

However, in characterising objective opportunities, Archer argues that these do not 'force' actors to do anything, but rather serve as 'reasons' for acting in one way or another (1995: 208). Archer states:

...as with any reason, agents have to find it good and material considerations are not the only motives to action. Nevertheless, as they weigh them in the balance, costs and penalties tip the scales in one direction, meaning that countervailing concerns would have to be strong enough to outweigh them. It is agents alone who do the weighing, who assign values to the weight of incommensurables, and determine the sacrifices and trade-offs that they can bear. (Archer, 1995: 209)

Given that objective reasons necessarily have a certain weight, what factors can counterbalance them? The key influence mentioned by Archer is that of altruism, which involves the renunciation of vested interests (Archer, 1995: 206). To act altruistically is to opt for some course of action even though it will cost one as an actor. As an example of this, Archer argues that those who are involved in caring professions would typically have been better off as accountants, but altruistically refuse the benefits of the latter in order to perform a moral duty (Archer, 1995: 210).

The difficulty that this raises for Archer is that she wants to account for altruistic behaviour as *both* an act of undetermined agency *and* a phenomenon that is sociologically explicable. Archer wishes to avoid the idea that altruism is based on an 'inexplicable whim', and views it as a reasoned choice that makes sense in certain cultural contexts.

As such, Archer states:

Most sacrificial activities are embedded in cultural belief systems which may be vastly more important to the individual than any other aspect of their social context, but are still not of a person's own making (the Christian martyr did not make Christianity). (Archer, 1995: 211)

Locating altruism as cultural belief, however, threatens to take away its character as a genuine act of renunciation. As we saw earlier, Archer argues that cultural beliefs are taken up and defended for the promotion of the material or ideal interests of actors involved. To be explicable on Archer's terms, subscription to an altruistic belief system must provide an

objective advantage to actors⁹. In this light, altruistic action looks less like the counterbalancing of objective factors by subjective values, and more like the result of weighing up different objective advantages in which those gained from upholding certain cultural beliefs win out. On such an account, there is no moral agency operating, just actors assessing objective structural opportunities to see which course of action will bring them the most benefit.

The problem with Archer's account is that her sociological analysis of structure and culture only gives an 'objective' account of behaviour that is oriented to accruing material and ideal rewards¹⁰. When Archer wavers towards locating moral behaviour structurally, her only viable option is to see it as self-interested. In contrast, Archer's attempts to give morality an agentic character make it impossible to analyse sociologically. Some analysts might find the latter option appealing, arguing that moral behaviour cannot be sociologically accounted for because it is based in the human capacity of 'free will'. However, there are good reasons to reject such a position. Firstly, within societies such as our own, there seem to be structurally explicable differences among those engaging in altruistic behaviour. Archer's example of the altruism of those in the caring professions exemplifies this point. As is well known, within twentieth century Britain these professions have been dominated by women. It seems unlikely that women's heightened moral commitment in this area can be explained by chance, for instance, by claiming that women just happen to exercise their moral agency more frequently than men. More likely, there are structural reasons accounting for this, such as the gendered split of the public (masculine) and private (feminine) spheres and the association of care with the private sphere. Secondly, it is hard to dispute that the forms of moral behaviour which humans engage in vary *between* societies. Rather than attributing this variation to the unpredictable operation of free will, we should attempt to offer structural sociological explanations of it. Useful work in this area has been done by the philosopher Alasdair MacIntyre who, in works such as *A Short History of Ethics* (1967) and *After Virtue* (1985), attempts to make forms of moral conduct intelligible by locating them in

⁹ There is a brief passage in *Culture and Agency* which suggests that actors may engage in cultural reflection and conflict which is not motivated by material or ideal interests (Archer, 1988: 284). Altruistic commitments could be placed into this category. However, Archer offers no analysis whatsoever of what might motivate actors to reflect on beliefs, commit to them, and defend them if this is not motivated by interests of some kind. In the face of an analytical juggernaut in which interests occupy the driving seat, such a remark seems under-revved. It certainly does nothing to make altruistic commitments any more structurally explicable, which is the key issue here.

¹⁰ Lilli Zeuner (1999) makes a similar criticism although she fails to note that the rewards in question might be 'ideal'. This omission is hardly surprising, given the undertheorised nature of 'ideal interests' in Archer's account.

their social context. Thirdly, placing moral behaviour in the realm of agency stops us from comprehending it as a need of human beings which is as 'objective' as the other needs and wants theorised by Archer. This blocks off from sociological analysis questions about the forms of social relations which best actualise the human need to act morally and the forms of moral behaviour which best meet this need. These questions are relevant to analysing both the distribution of moral behaviour within a society and the differences in behaviour across societies. Contrary to Archer's approach, treating morality in this way provides a structural analysis of morality without reducing it to self-interested behaviour.

So far we have been considering the difficulties that arise when Archer analyses moral agency. However, there are also problems with her use of the notion 'interests'. As we have seen, Archer argues that social structures define what is in the objective interests of actors, and operate regardless of actors' understandings. Archer writes:

Since positions on social distributions concern scarcity, then there is a bonus to be lost with ceding a high position and penalties to be shed by not acceding to a low one (compare the effects on life-chances of downward or upward mobility through marriage). Those who fail to recognize this, or are induced to mis-recognize it, pay the price uncomprehendingly, but quite objectively in terms of a worsening of their situation or a perpetuation of underprivilege. (Archer, 1995: 206)

This kind of argument is consistent with Archer's realism, which emphasises the material nature of structures and their causal influence independent of actors' belief. It provides the warrant for a scientific analysis of society whose task is to describe the characteristics of these structures. Nevertheless, having established the objective reality of structures, Archer then works to undermine this.

One way in which this occurs is Archer's distinction between 'objective' and 'real' interests. For Archer, the objective advantages or disadvantages that inhere in structural positions are 'vested interests', which are the substance of her sociological analysis. However, Archer argue that 'vested interests' are not to be confused with 'real interests'. Writing of vested interests she states:

...they are wholly objective; they are not to be confounded with agents' mental states...nor do they stand in any particular relationship to anyone's real interests. (Despite the difficulties entailed in defining the latter, we can still conclude that it *might* not be in the real interests of the idle rich to perpetuate their idleness, while it is certainly in accordance with their vested interests.) In other words, agents' vested interests are objective features of their situation which, if it will be maintained, then predispose them to different courses of action and even towards different life courses. (Archer, 1995: 203. Author's emphasis)

Coming from an avowed realist, the idea that there might be 'real' interests that are distinct from and potentially contradictory to those that are the 'objective' substance of analysis is an odd one. Surely, if real interests existed, they would share the character of vested interests and exert a conditional influence on actors as an 'objective feature' of their situations, likewise predisposing them to certain courses of behaviour? If this was the case, however, a contradiction between real interests and vested interests could not be analysed as if only the latter conditioned behaviour. Archer's treatment of real interests contravenes realism by asserting that they need not be incorporated into a structural analysis even if they contradict the existing processes that are postulated. The ultimate implication of Archer's analysis is that people can act against their real interests unproblematically, but will suffer penalties if they act against their vested interests. Quite what reality and influence the former have is then open to doubt.

Problems pertaining to the unreality of structures also emerge in Archer's separation of the system and social levels. As we have seen, the system level specifies the resources that are available to actors for the pursuit of their projects. It also outlines the objective characteristics of institutional structures and their relations of compatibility or strain to one another. These characteristics influence the possibilities for actors, as action that calls on the resources of compatible institutions achieves its goals unproblematically, whereas action calling on conflicting institutions produces practical problems for actors (Archer, 1995: 215). All of these structural features are characterised by their causal influence and necessity.

However, it is also essential to Archer's analysis that these structures do not operate alone, but only work in combination with human agency. Crucially, it is possible for human agents to cancel out the influence of these objective structural features. Archer states that:

People, in turn, are capable of resisting, repudiating, suspending or circumventing structural or cultural tendencies, in ways which are unpredictable because of their creative powers as human beings. In other words, the exercise of socio-cultural powers is dependent *inter alia* upon their reception and realization by people...(Archer, 1995: 195)

Having insisted on the reality of structural influence, Archer then argues that this can be suspended or circumvented by actors.

The separation between system and social levels is a version of the science/society division, in which social life is said to involve agency as well as objective structures knowable by science. Archer thus attempts to find a place for agency as well as structure, but it is hard to see what independent contribution the latter can make. As Holmwood and Stewart point out in relation to the social/system distinction, statements of the system level

are held to be scientifically justifiable claims about the most resourceful options for actors. If this is the case, then forms of social organisation which do not fit with system possibilities must necessarily be suboptimal (Holmwood and Stewart, 1991: 162-5). However, Archer argues that 'social' deviation from a particular 'system' statement need not be suboptimal, but can have a positive character because of its inspiration in the reflexivity of actors. Considering an example of the 'independent variation' of parts and people will help to clarify why this is not the case.

One instance of a system/social disjunction used by Archer relates to the 'poor of modernity' who include the young unemployed, the old-aged, the homeless, single parent families, and the 'handicapped' (Archer, 1995: 185). She argues that these groups share the condition of poverty generated by 'late welfare capitalism' but do not have a sense of their common lot and common interests (Archer, 1995: 185). In other words, this is an example of a lack of fit between the system level, on which these groups share an interest, and the social level, on which they do not organise together to promote themselves. To explain this lack of social cohesion Archer states:

...the members of this collectivity [the poor of modernity] are more reflexively concerned with their differences than their similarities. Thus generational differences divide the young unemployed from the old-aged (two of the largest categories of the poor), ethnocentrism erects a racial barrier to cohesion, whilst the handicapped, homeless and single parent families increasingly pursue their interests through special interest groups rather than by more generalized forms of collaboration. Social affinities and antagonisms thus fuel fissiparousness: they do not preclude the development of Corporate agents, but mean that these will be in the plural (addressing single-issues) rather than in the singular (confronting the plurality of vested grievances shared by the underprivileged). (Archer, 1995: 185)

The use of the notion of reflexivity is crucial here. While in general statements of the system/social distinction Archer had argued that reflexivity allowed a 'creative' response to structural possibilities, in this passage the reflexive concerns of actors are precisely what gives them a suboptimal response to their interests. In other words, it is their reflexive sense of differences that blocks them from organising together to pursue their 'true' interests. Thus, agency has not suspended or circumvented structural influences but represents a failure to grasp structural potentials which results in disadvantage to the actors involved. To this extent, Archer's specific example subverts her general claims about agency. Agency does not represent creativity so much as suboptimality¹¹.

¹¹ This is the same problem that emerges in Giddens' discussion of agency. Giddens insists that agents can choose how they respond to situations, but cannot explain how a choice to diverge from the most successful known option can be reasonable for actors.

In order to preserve the system/social distinction, Archer has to then argue that this suboptimality will not be a motivating feature for actors to reorganise themselves. As we have seen, she argues that because of their reflexive differentiation from one another, groups affected by poverty will not organise together in their shared interest. This claim is problematic, because it suggests that the groups that presently exist will not change their boundaries. In fact, when it becomes known to these groups that they share a common interest, it seems plausible that this will reconfigure their 'social' sense of difference and similarity. Archer's argument that this will not happen is based on an assertion of the social differences between these groups, but she does not explain why these differences existed in the first place nor, more importantly, why they should be sustained in the face of knowledge of common interest¹². To retain the system/social split, Archer has to reify the current, problematic, understandings of actors about where their interests lie.

This problem arises in another form in Archer's argument that there are two levels of 'interests' to be considered in social analysis, invoking the science/society division once again. One set of interests is based on the relations between scientifically knowable structures of society that are drawn on in action. In institutional frameworks where structures cohere with one another, actors can pursue their goals without problems. Likewise, where structures contradict one another, activity is disrupted. It is therefore in actors' interests to support coherent institutional structures, in parallel with the coherence sought by natural scientists in their attempts to interact successfully with the material world. However, Archer argues that there is also a 'social' logic of vested interests, where actors' interests are understood in terms of 'relative advantage' rather than 'absolute well-being', implying the necessity of competition between actors (Archer, 1995: 204). If actors are in a relatively advantageous position in society, they have the motivation to maintain a social system; if they suffer from relative disadvantage, they have an interest in change. Thus, social life is said to involve certain interests that are not grounded in a successful relation with the environment, but in the strategic/competitive nature of society.

I would argue that this dual logic cannot be sustained, and that the extra 'social' interests postulated by Archer cannot be incorporated into a defensible analysis of society. This is

¹² In fact, her claims play on the implicit notion that these groups actually have different interests bestowed on them by the system level, which would then explain their social differentiation. However, this contravenes Archer's initial premise that the groups actually share an interest. As it happens, the idea that these groups have different interests is more plausible than the claim that their interests are shared because they are all poor. It seems likely that the interests of these groups would be served by different, potentially incommensurable, kinds of change, which suggests that there is a structural basis to their social sense of differentiation.

illustrated by Archer's analysis of the institutional framework that she terms 'necessary complementarities' (Archer, 1995: 219-221). In this situation, institutional structures mesh with one another and show a high degree of coherence in their operation, facilitating action. As an example of this, Archer refers to the social system in 'Ancient India' in which institutions of caste, religion, kinship, polity and law are highly compatible with one another. Archer admits that such a system creates a 'relatively privileged group' in the Brahmin caste, as well as a hierarchy of caste groupings. However, she also argues that 'everyone has something to lose from disruption' and so all agents act to protect the system rather than challenging it (Archer, 1995: 220). Thus, every caste group is supplied some benefits by members of a subordinate caste, and these benefits would be lost if caste differentiations were not maintained (Archer, 1995: 220-1). Furthermore, attempts to challenge the system result in individuals becoming untouchable and losing benefits from the system (Archer, 1995: 221). In essence, all actors have a vested interest in stability¹³.

Archer's claims involve an analytical sleight-of-hand on her part to conceal the problems that this example raises. In order to make a consistent analysis, she has to deny that the hierarchical nature of the caste system gives rise to a group of actors with a vested interest in change. This is because Archer's argument that the institutions are coherent means that actors' goals are being met, implying system stability. If it were to be admitted that the caste system also involved inequality, this would imply that the system both satisfied actors (because of its coherence) and didn't satisfy actors (because of its inequality) at the same time. Thus, Archer has to suggest that all actors have a vested interest in stability, but this contravenes her general account of what such interests entail. Archer shifts the meaning of an interest in stability from being 'relative advantage over the majority of actors' to being 'relative advantage over somebody'. Of course, possession of the latter does not entail possession of the former, and when analysed it must turn out that, on Archer's initial definition, a large proportion of actors have a vested interest in change because of their relative disadvantage in general societal terms¹⁴. Certainly, we can accept that individual 'deviants' from the existing order face the penalty of dropping into the untouchables, but this does not mean that a large organised group of the disadvantaged could not avoid this outcome. Organising resistance may not be straightforward. However, whatever the difficulties, in Archer's terms actors still have a 'vested interest' in finding a way to

¹³ All actors except the untouchables, that is, although Archer does not make this explicit, or discuss their attempts to challenge the social system.

¹⁴ To insist that relative advantage is to be understood to mean 'relative advantage over somebody' would be to argue for the stability of all social systems given that nearly all members of a society have relative advantage over *somebody*.

challenge the system. Archer resists this proper working through of her logic because the result is analytical confusion. She is committed, ultimately, to arguing both that a social structure facilitates actors' interests (because of its coherent institutions) and does not (because of its hierarchical nature).

This difficulty is another consequence of the science/society division, which is here present in Archer's attribution of dual interests, arising both from structural configurations and a more general competitive logic of the social. I would argue that actors' interests can be more plausibly located by identifying them only at the structural level, which can account for both unity of interest and conflict of interest. Such an analysis would examine how problems and tensions within institutional structures generate divisions of interest between actors rather than assuming, as Archer does, that such divisions must inevitably exist at the 'social' level. Thus, if the caste system produces conflicts of interests, the basis of these must be located within its institutional structures, instead of suggesting that such structures are 'coherent', allowing all actors to achieve their goals unproblematically.

Although the empirical details of Marx's analysis have turned out to be erroneous, his mode of analysis is an example of the general approach recommended here. Marx argues that divisions of interest arise from particular modes of production, rather than existing as a necessary feature of all human interaction. His arguments are oriented to demonstrating why capitalist institutions generate divisions of interest, and how these divisions can be overcome by the transformation from capitalism into a socialist mode of production. It seems almost certain that Marx did not correctly locate the problems and tensions existing in the social structures of modern European societies. However, his attempt to institutionally locate social divisions and find a way to transcend them is exemplary of how social science can proceed. By arguing that divisions of interest must always exist on the social level, Archer hobbles such a possibility from the very beginning.

3.4 Culture, structure and interests

So far, we have been considering Archer's analysis of social structure, and the problems generated by her separation of the objective characteristics of structure and the subjective apprehension of these by actors. This is not the end of the problems with Archer's account, however, and in this section I want to consider various difficulties that arise from Archer's attempt to separate issues of 'culture' from those of 'structure' and 'interest'. Archer argues that all items of culture with propositional content exist in the Cultural System. This is then separated off from the constituents of the Socio-Cultural level which are social groups with

material and ideal interests to pursue. This whole sphere is then separated again from that of 'structure' which contains the objective social structures dealt with by social actors, and the properties of agents that are independent of structure. The architectural principle behind Archer's system is that the qualities of these various levels can be specified independently of one another. Unfortunately for Archer, this edifice turns out to be an unstable one.

In the first place, Archer's analysis requires that the interests of actors (residing on the Socio-Cultural level) can be separated from the constituents of the Cultural System so that interests can be specified independently of the logical relations of beliefs. However, it seems plausible to see 'interests' themselves as elements of the Cultural System. In order to be counted as part of the Cultural System, an item has to be:

- (i) intelligible; that is 'capable of being grasped, deciphered, understood or known by someone.' (Archer, 1988: 104)
- (ii) a proposition which asserts truth or falsity. (Archer, 1988: xvi)
- (iii) an item to which 'the law of contradiction can be applied.' (Archer, 1988: xvi)

In relation to the first, if actors are to be motivated by their interests then they must have some understanding or comprehension of these. Archer's account of Socio-Cultural interaction is based upon the idea that actors pursue their interests, and unless they do so unconsciously, then these interests must be formulated as understandings¹⁵. This suggests that interests meet criterion (i) for entry into the Cultural System. Statements of interest must also assert the benefit for actors of pursuing a certain course of action, such as: 'It is in the interests of the Conservative Party to defend hereditarianism'. Such statements are propositions¹⁶ which assert something to be true, meeting criterion (ii). They are also items to which the law of contradiction can be applied. For example, the above proposition is contradicted by the statement: 'It is not in the interests of the Conservative Party to defend hereditarianism'. As a result, they meet criterion (iii), and must therefore be included in the Cultural System¹⁷.

¹⁵ One of Archer's own examples illustrates this point. She discusses the situation where a social group realises that they are not benefiting from some cultural configuration. Archer writes that such a group 'makes comparisons with other social groups with which it interacts and can hardly fail to note that some of its competitors are beneficiaries of cultural support whereas their own promotive efforts attract cultural opprobrium' (Archer, 1995: 314). Clearly, what is taking place here is a conscious assessment of what is in the groups interest.

¹⁶ Or can be formulated as propositions by the insertion of relevant specifications (time, location etc.). All that is required for entry into the Cultural System is that this can be done.

¹⁷ It might be said that this argument elides the distinction between interests and their statement. However, it seems reasonable to assume that unless actors have some 'direct' and non-cultural apprehension of their interests, it is in fact their understandings of their interests that motivate their action, and not the interests 'in themselves', whatever they might be.

This undermines Archer's attempt to specify interests independently of the Cultural System and explain morphostasis and morphogenesis by the variation between logical relations and interests. Her model explains cultural developments by analysing how beliefs are defended or challenged by actors pursuing interests that are external to the Cultural System. However, because interests have to be taken as beliefs within the Cultural System, this mode of explanation cannot be sustained. Interests, instead of varying independently of the objective logic of belief, are intimately bound up with the latter. If interests are beliefs, then their uptake would surely have to be affected by questions of their logical contradiction or coherence. Likewise, those beliefs relevant to statements of interest would be influential not only by their logical relations, but by whether or not they offered viable statements of what will benefit actors¹⁸.

This points to wider problems in Archer's account of the relation between culture and structure. Archer wants to separate out these spheres in order to analyse their interconnection. However, the influence of structure cannot be understood without being given a cultural apprehension, and the importance of logical relations between theories and beliefs cannot be explained without considering how this relates to resourceful activity. If we take the issue of structure first, Archer's mode of argument is to insist on the causal influence that structures have regardless of the interpretations placed on them. She states:

...why do the effects of famine, conquest, of a demographic structure or an income distribution require reading? Their objective influence may be to leave many dead, enslaved, poor or disadvantaged, in a way which can be consequential in itself, could be independent of their having readers (nuclear holocaust), and often have an efficacy regardless of any readings which are placed on them...(Archer, 1995: 112)

Archer is correct to argue that there are some causal influences which have an impact no matter how they are 'read' or 'interpreted' by actors. However, whenever social scientists or lay actors attempt to understand such influences, these understandings are interpretations which are subject to assessments of validity. Statements of structure are thus cultural, and to argue otherwise would be to reject the entire thrust of post-positivist philosophy of science. Oddly enough, Archer seems to recognise this in relation to natural science. In *Culture and Agency*, she argues that both natural scientific theories and the empirical 'facts' that they deal with are theoretical and are elements of the Cultural System (Archer, 1988: 151-3). The argument that facts 'speak for themselves' is taken to represent the 'fallacy of empiricism'

¹⁸ It should be noted that this differs from Archer's claim that actors take up beliefs which will advance their interests. Archer's claim implies the externality of 'belief' and 'interest' whereas here the belief itself is a statement of interest.

(Archer, 1988: 151-3). Thus, Archer recognizes that there can be no non-cultural account of the structures of natural science. Logically speaking, we would expect the structures of the social world to be treated in the same way, because their character as structures is based in their materiality and necessity, i.e. the same features as structures in the natural world. In that she analyses social structures outside of the cultural realm, Archer inverts the normal contrast between natural and social science, implying that the structures analysed by the latter have a more direct determining force than those of the former. It is surely more plausible to argue that in both the natural and social sciences our access to structures is indirect, and is mediated by our cultural understandings. This foregrounds questions about the validity of the analyst's account of structure, removing the problematic implication that this account captures the characteristics of structures as they are 'in themselves'.

The converse difficulty emerges for Archer in her analysis of culture, and her attempt to capture the 'force' of logic. Archer goes to great lengths to show that logical categories are cross-culturally applicable, and she moves from this to the claim that logical relations of belief have a 'causal influence' upon actors. However, Archer does not attempt to explain why beliefs about logic are prevalent, nor the source of their apparent forcefulness. It may appear as if her emphasis on issues of legitimacy offers such an explanation. Archer argues that actors frequently subscribe to beliefs in order to claim legitimacy for their ideal or material interests, and if actors hold contradictory beliefs then this legitimacy is undermined. Unfortunately, the connection of coherence and legitimacy does not account for the power of logical relations, but merely reflects it. We can agree with Archer that coherent belief is taken to be legitimate, but this gets us no further in explaining why this, rather than the reverse, is so.

A plausible reason for Archer's failure is that an explanation of the power of logical relations requires that culture is not separated from structure. Archer tries to split questions regarding the adequacy of belief (logical relations) from questions of adequate interaction with the material environment (structural relations). However, it is only by connecting culture with the realisation of practical goals that we can account for the power of logical relations such as coherence and contradiction. In respect of natural scientific theories, a coherent theory is powerful because it allows us to successfully account for processes in the world. This contributes to our ability to respond adequately to these processes and produce outcomes that we desire in our relations with them. Likewise, if a theory is contradictory, then some aspects of the process it theorizes are unintelligible and unpredictable, limiting human ability to deal successfully with it. For example, although offering a powerful

account of the motion of planets, Kepler's laws came to conflict with later observational accounts of the movements of Saturn and Jupiter. This contradiction meant that certain aspects of planetary motion could not be accounted for using Kepler's laws, and were thus unintelligible and unpredictable until Newton's law of universal gravitation made the observed variations explicable by theorising the forces of attraction between planets (Pannekoek, 1961: 261-281). By restoring the coherence between the broader theory and observational accounts, Newton's work made the processes in question more intelligible and predictable¹⁹.

The nature of relations between beliefs is as important in other aspects of human activity as it is in natural science. The pursuit of human goals using contradictory beliefs, or the pursuit of contradictory goals, results in outcomes that do not conform to the intentions of actors. The former case is directly analogous with substantive questions in science in which contradictory understandings lead to unpredictable results. To illustrate the latter, one could argue that it is not possible to achieve both a continuing increase in the standard of living of Western nations and the preservation of the natural environment. Successful achievement of one goal necessarily undermines successful achievement of the other. The relations of contradiction or coherence between beliefs thus have force because they relate to human capacities to interact with the environment and achieve desired goals²⁰.

The failure of Archer to connect issues of cultural coherence with practical success is demonstrated in her treatment of the logical relation of 'constraining contradiction'. In this situation, a social interest group 'a' invokes theory A, which requires theory B to operate, but is contradicted by it. For Archer, if the social group is powerful, it will be to their advantage to suppress knowledge of this contradiction, so that other social groups do not learn of it and use it to undermine group 'a' (Archer, 1988: 189-197). This involves the claim that it can be in the 'interest' of a social group to hold contradictory beliefs so long as no competing groups become aware of this contradiction. However, if we reconnect issues of logical contradiction with practical activity, it is dubious to argue that it can be in the interests of a group to continue holding contradictory beliefs, even if other actors are not aware of this. Rather, the employment of contradictory beliefs in activities would undermine the interests of the group because it would lead to their non-fulfilment. Either the group

¹⁹ Of course, as is commonly noted, it is not only the broader theory which may be subject to reformulation when there is a clash between a theory and an observational account. Both of these are theories and subject to reformulation when necessary.

²⁰ This suggests a reason why coherence is associated with legitimacy: if a social group can give a coherent account of why the pursuit of their beliefs and goals will be to the benefit of all, then this provides such a group with legitimacy.

would be unable to achieve their ends because of contradictions in their knowledge of means, or they would be unable to achieve all of their desired ends because the ends contradicted one another. To have an 'interest' in contradiction would be to have an 'interest' in error, which surely goes against the scientific ethos of Archer's realism.

These arguments indicate that there are serious problems with attempting to separate structure and culture. Giving a non-cultural account of 'structure' implies that the analyst has some form of direct, unmediated contact with the world. Likewise, a non-structural account of 'culture' separates the force of relations between beliefs from the practical outcomes of employing such beliefs. It thus cannot explain why logical relations have any force at all.

3.5 Conclusion

To conclude this chapter, I would like to consider further how this analysis of Archer's work relates to the wider themes of the thesis. Archer's insistence that the analysis of society must be separated into the components of structure and agency is a clear example of a dualist approach to science and society. For Archer, the structures apparent in social life are of the same kind as those discovered by natural science. Structures have the characteristics of materiality and necessity, and their operation can be identified through scientific investigation. What is special about society, for Archer, is that human beings have agency, which operates as well as structure. A realist analysis of society thus proceeds by theorising the interplay of its two constituents, objective structure (the scientific element) and subjective agency (the social element). It is crucial to note here that society's distinctive feature is that it involves something in addition to that which can be analysed scientifically; the extra 'social' factors being considered are therefore those elements that are not scientifically explicable. If we take science to be the measure of success in interactions with the material world, as realists do, then the social must consist in those factors which are not related to success. Nevertheless, Archer attempts to give a positive characterisation to this extra social dimension, suggesting that it encompasses morality, reflexivity and strategic action. I have argued in this chapter that none of Archer's claims for a positive divergence from structure can be born out. Her account of moral agency made it appear inexplicable or self-interested, and a structural analysis provides a better account of the existence of moral action. Archer's account of social interests that 'reflexively' diverged from the objective logic of the situation was equally problematic, and if such interests were indeed pursued, they would result in suboptimal outcomes for the supposedly reflexive actors. Likewise,

Archer's association of the social with strategic competition could not be made coherent with analysis of the possibilities provided for actors by structure. In summary, Archer fails to locate a positive or even neutral extra dimension to social action over and above successful relations with the environment. Any divergence from the possibilities contained within structure turn out to be inexplicable or suboptimal.

It is important to recognise that this is the same problem that emerges in the work of Giddens. Archer attempts to clearly distinguish the realist perspective from Giddens' anti-naturalistic approach, on the basis that realism involves a scientific investigation of the material structures of social life (Archer, 1995: 108-117). Nevertheless, Archer shares Giddens' claim that human agency gives society special features beyond those of material structures that are knowable through science. As with Giddens, Archer fails to give positive substance to these extra features, which are ultimately associated with an unsuccessful relation to structure. Rather than providing a genuine alternative to Giddens' work, Archer's theorising shares the problematic division between science and society.

This division also has consequences for the term on the other side of the dualism, resulting in a problematic conception of science. I have already pointed out that by separating structure from culture, Archer implies that understandings of social structure can be somehow non-cultural. There is, however, a more general difficulty with Archer's account of the objective features of analysis, that applies to both the structural and cultural spheres. Archer suggests that in each sphere, the objective characteristics (of structure or logic) be separated from the subjective or social apprehension of them. Analysis then proceeds by theorising the interplay of the two levels. Archer does not acknowledge that this requires the social scientist to have a non-social apprehension of the structures that presently exist²¹. Accounts of whether structures and beliefs²² are coherent or contradictory are presented as if these were final and unchallengeable products of scientific investigation, which is conceived of as offering a completed and unproblematic account of the existing objective relations.

This problem connects to the difficulties identified in Bhaskar's realism, considered in Chapter 1. It will be recalled that Bhaskar distinguishes the real and the actual, the real being the level of generative structures, and the actual being the level of events. Bhaskar suggests that agency may produce differences between what is structurally explicable (on the

²¹ Of course, Archer suggests that new institutions and beliefs may be generated, which would necessitate an expansion of the stock of knowledge. Nevertheless, this doesn't alter the finality of claims about current institutions and beliefs.

²² As I argued above, strictly speaking it does not make sense to separate the objective logics of structure and belief. These are combined in the analysis of accounts of structure and their consequences.

level of the real), and what occurs on the level of events (the level of the actual). I argued that this allows current understandings of structure to be reified, as events which cannot be structurally explained can always be attributed to agency, meaning that structural accounts can be treated as fully adequate. It is this reification that links the realist accounts of Archer and Bhaskar. Just as Archer's division of structure and agency implies that science can show structures as they truly are, Bhaskar's division of the real and the actual allows claims about structures to be taken as final. Although neither overtly sanctions an empiricist conception of science in which theories have 'direct' contact with the world, each produces an account which allows structural statements to be interpreted in this fashion.

As an alternative to this form of realism, I would suggest that scientific theories are theoretical mediations, the 'structures' that they postulate being the positive categories of understanding. This means that accounts of 'structures' are subject to reconstruction in the process of investigation, in which the adequacy of theories is assessed. Anomalies cannot be ignored by assigning them to 'agency', but must be dealt with by transforming accounts of structure to improve theoretical adequacy. Such an approach suggests how the two terms of Archer's dualistic analysis can be transcended. Instead of a foundational notion of science and a conception of social activity that excludes issues of success, both scientific investigation and other social activities should be understood as historically located practices oriented to successful relations with the material world.

Part III

Science and Society Reconciled?

New Sociology of Science

Science as a Social Construction: The Strong Programme and Success

4.1 Introduction

In Part II of the thesis, I argued that the dualism of science and society is present in both naturalistic and anti-naturalistic sociological theories. Part III is dedicated to an exploration of whether sociological accounts of science, having a direct concern with the interrelation of science and society, avoid a problematic division between the two. The sociological analysis of science has a long history, and there are many different approaches to its study, reflecting the diversity of approaches within sociology itself. Nevertheless, a crucial turning point in this history was the attempt to offer a completely 'social' analysis of scientific investigation, including a sociological explanation of the content of scientific theories. This is to be found in the work of the Barry Barnes and David Bloor, who developed the 'Strong Programme' approach in the 1970s. Barnes and Bloor were concerned that earlier analyses had a restricted sense of what was 'social' in character, placing limits on what could be sociologically explained. Previous analyses had either focused largely on the social character of scientific institutions (as in the case of Merton and his followers) or considered only certain kinds of knowledge to be social in nature (as in the case of Mannheim (1936)). The effect of this in both approaches was to retain a science/society division, separating what was socially constituted (whether institutions or certain kinds of cultural belief) from what was not (valid scientific knowledge, logic, mathematics)¹. In essence, these approaches held that valid scientific knowledge developed through a logic distinct to that of social processes, and could not be explained in the same way as the latter. This view was encouraged by those philosophers who wished to separate out that which could be 'rationally' accounted for in the

¹ Both Mertonian and Mannheimian approaches have been defended by adherents responding to the Strong Programme criticisms (see Gieryn (1982) and Pels (1996b) respectively). Although they usefully emphasise the subtleties of these modes of analysis, they also reinforce the sense that such modes are dualistic in orientation. Gieryn argues that a Mertonian approach avoids 'sociological reductionism' and analyses knowledge by considering inputs from both the natural world and social institutions (Gieryn, 1982: 287-9). Likewise, Pels claims that Mannheim's work involved a 'productive indecision' in which reductionist accounts emphasising only the cognitive or the social were rejected (Pels, 1996b: 41-3). In other words, these authors argue for a combinatorial approach which retains the dualism of science and society.

history of science from that which was amenable to a sociological explanation (Lakatos, 1978; Laudan 1977). Barnes and Bloor rejected these restrictions on sociology's scope, offering instead a fully social account of all belief, including scientific knowledge. In their view, all understandings, whether considered true or false, reliable or unreliable, are to be treated as sociologically explicable. There is no sphere of rational belief that is untouched by social processes. Rather, all beliefs are 'socially constructed', made from the fabric of the social.

This chapter examines the Strong Programme position, taking it as exemplary of social constructionist analyses of science. There have certainly been important developments in the sociological analysis of science since the Strong Programme's early manifesto statements. Nevertheless, the Strong Programme arguments provided a crucial foundation for analysis in the sociology of science, and their claim that science is a fully social activity is considered by many to be unshakeable. Much important work in the area has been done by analysts who were closely affiliated with the Strong Programme (see for example Steven Shapin (Shapin and Schaffer, 1985), Donald MacKenzie (1981), Andrew Pickering (1984)) or defended positions with similar characteristics (see for example Harry Collins (1985), Trevor Pinch (Collins and Pinch, 1982)). Furthermore, even those who challenge the Strong Programme often accept the notion that scientific knowledge is socially constructed. For example, reflexivists such as Steve Woolgar (1988a) and Malcolm Ashmore (1989) are in agreement with constructionist claims about knowledge, but attempt to apply this insight back to sociological analysis itself. I will consider one of the key breaks with constructionist analysis, actor-network theory, in the next chapter. This chapter is dedicated to a consideration of the social constructionist approach and its limitations.

The attempt by Barnes and Bloor to conceive of natural science as a fully social activity was an important and useful development in the analysis of science. Furthermore, their explicit commitment to an instrumentalist analysis suggested that their sociological theorising would be combined with a defensible philosophy of science. However, I shall be arguing that the Strong Programme approach fails to offer a consistent account of science and society. The instrumentalism espoused by Barnes and Bloor is undermined by their mode of analysis, which ends up removing consideration of the variable success of belief from the explanatory system. This is a consequence of the notion of 'the social' employed by the Strong Programmers. Although their approach avoids dividing science from other social activities, this does not mean that they avoid the problems of the science/society dualism. The reason for this is that Barnes and Bloor do not go beyond the division by

transforming both terms, attempting instead to subsume scientific investigation under the existing conception of the social. In the dualistic conception of science and society, scientific knowledge is claimed to be rationally founded and successful, and issues of variable success are omitted from an analysis of social activities. Barnes and Bloor invoke this conception of the social, suggesting that all institutionalised understandings, whether natural scientific or otherwise, are equally valid. In contrast, I would suggest that a genuine analytical reconciliation of science and society should accept the social character of all activities without omitting analysis of their variable success.

This chapter considers the difficulties with the Strong Programme (SP) analysis of scientific investigation and other social practices. In relation to science, the focus is on theories of classification, and their sociological consequences. I begin by considering the SP account, termed 'meaning finitism', which is intended to make space for a sociological explanation of the categories used by science. I argue that this approach fails because it does not properly account for the success or otherwise of classifications. I then consider the application of the SP approach to other social institutions, and argue that the failure to incorporate issues of success is as problematic here as it is in accounting for science.

4.2 Theories of Categorisation

This section addresses the SP theory of categorisation. This may seem like an odd issue to focus on, but it is, in fact, central to our understanding of scientific knowledge. As Barnes and Bloor realise, by examining how categorisations operate, we can reflect on the important issue of the justification of this knowledge. A consideration of processes of categorisation raises two versions of the same epistemological problem. Firstly, to place a group of particulars² under a category is to assert their similarity in some respect. For example, if a group of objects are categorised as 'red', it is being asserted that they are similar in colour. The important epistemological problem is to what extent this claim to similarity can be justified. Can it be demonstrated that the categorisation is an appropriate one, and that the similarity that is claimed actually holds? Are all of the objects best categorised as 'red', or might some of them fit a competing categorisation equally well? The other version of this problem relates to the application of existing categorisations to new particulars. The issue is whether categorisations provide clear distinctions, so that new particulars can be unproblematically classified. Can it be demonstrated that a new particular is an instance (or

² The term 'particular' refers to any entity (animate or inanimate), process or characteristic that can be delineated and considered for classification.

is not an instance) of an existing category? If the category 'red' is applied to a newly encountered table, can this claim to similarity be justified?

When these questions are spelled out, they seem dauntingly abstract. Nevertheless, the Strong Programmers (SPers) attempt to provide an answer to them. It seems accurate to say that their epistemological perspective is shaped by the desire to make room for a sociological analysis of categorisation processes. In order to do so, Barnes and Bloor build upon meaning finitism, an approach to the meaning and content of categorisations developed from the work of Mary Hesse and Ludwig Wittgenstein among others³.

Meaning finitism is an account of categorisations that is intended to apply to all categorisations, whether these are part of our wider culture or lodged in specialised bodies of knowledge. In that it analyses the justification of categorisations, its consequences spread through to all forms of knowledge that call on these, including generalisations, laws, predictions, and so on. The theory focuses on acts of categorisation, and the processes whereby particular objects or processes found in the world are inducted into categories. It argues that the 'meaning' or 'content' of any term is created by associating it with a set of instances which that term exemplifies. To see how this occurs we must examine the processes of inductive learning by which categorisations are generated.

The simplest case of learning a category is where a teacher uses ostensive definition to demonstrate to a student the contents of some concept. Ostensive definition is a process whereby the teacher points to a particular object and states the name of the category to which it belongs. So long as all goes well⁴, this indicates to the student how the object is to be classified. The content of this category is elaborated by the exposure of the student to further instances (repeated induction), until they have acquired a categorisation containing a number of members. For example, we can imagine a teacher demonstrating the category 'dog' by wandering around a park with a student and indicating those particular items that should be classified as dogs, such as an Alsatian, a Chihuahua, and so on. If the teaching is successful, the student builds up a set of instances of 'dog'. For meaning finitism, this is how a sense of similarity is built up. By categorising instances under the same term, a teacher indicates that these instances are all 'the same' in some way, that is, they resemble one another (Barnes, Bloor and Henry, 1996: 50). This marks them out as different from particulars that are

³ The meaning finitist approach and its sociological development are laid out in a number of different places, including Barnes (1981a), Barnes (1982), Barnes, Bloor and Henry (1996), Bloor (1997a). The essentials of the position are present in all of these expositions, with no significant divergences.

⁴ That is, some clarification may be required as to what exactly was being indicated, by distinguishing it from the backdrop in which it is placed, and from other possible aspects that might be inducted instead (for example, a learner may induct fluffiness from an attempt to indicate rabbithood).

members of other categories, such as those that are members of the term 'cat'. On a finitist account, then, the meaning of a category is constituted by the finite set of instances that it encompasses, which are designated as 'similar'.

Up to this stage, matters are fairly straightforward. So long as already existing categories are being taught, a teacher can draw upon the sets of instances which they know in order to indicate which particular belongs in which term. However, according to meaning finitism, matters are more problematic when it comes to categorising 'new' particulars that have not been encountered before. When a new particular is encountered, one would expect that existing concepts would provide guidance as to how this particular should be categorised. However, the SPers argue that, formally speaking, existing categorisations of 'sameness' do not provide any guidance whatsoever for continuing acts of classification, that is, attempts to decide if a new case fits under some term. As they put it:

Any classification of the next case can be reconciled with what has already been learned ostensively; however other things have been classified so far, the next thing can be classified in any way without any formal inconsistency with earlier practice. (Barnes, Bloor & Henry, 1996: 51)

The reasoning behind this move is fairly simple. The authors argue that the perception of 'resemblance' which allows us to classify one particular object in the same way as another (e.g. as two instances of 'dog') is always a relation of both sameness and difference. Any one instance of the category of dog is both similar to, and different from, other dogs (Barnes, Bloor & Henry, 1996: 50). The SPers then claim that this kind of resemblance (both sameness and difference) exists between any and every pair of particulars (Barnes, 1982: 28). We might want to object that we can still categorise particulars by their greater or lesser degrees of resemblance. However, Barnes, Bloor and Henry argue that there is no possible '*metric* for resemblance' and this being the case there is no objective basis for claiming that 'sameness here outweighs difference there' (1996: 51). Every particular resembles every other particular, and there is no way of measuring the degree of similarity between them in order to establish the formal validity of a categorisation. Whenever we try to decide whether a new particular should fall into category C, we find that there are grounds based on C's current membership to classify the particular as a C or a not-C (Barnes, 1981a: 313). In other words, there is no sufficient basis that can be drawn from the existing members of C to assert whether a new particular is an instance of C or not. Putting it in the terms used in Chapter 1, the act of categorisation is underdetermined by the available evidence, and no decisive claim can be made. To return to our example, even when a student is well-versed with the instances that we classify under 'dog', this categorisation does not supply enough

information for them to unproblematically categorise new, previously unexperienced particulars as 'dog' or not-'dog'. Each new particular will be like a 'dog' in some respects and unlike a 'dog' in others, providing no solid basis for categorising them either way.

One response to this finitist claim would be to argue that the meaning of a term is not merely in its set of members, but also in its relations to other terms. Such relations might then be used in order to evaluate whether a new particular can properly be assigned to a set, cutting down the indeterminacy claimed by finitists. Certainly, the finitist position accepts the relevance of other classifications to the meaning of a term (Barnes, 1981a). Such relations can be of various kinds. A term may be related by being connected through a generalisation such as 'dogs have paws', linking the membership of one set (dogs) with the possession of an attribute defined by another set (paws). In another kind of connection, learning the meaning of a term involves learning that of a contrasting but similar category. So, our learner who is being taught the meaning of 'dog' may attempt to classify Tiddles the tabby-cat as a dog when they try out their understanding of the category. To indicate more precisely what a dog is the teacher will point out that although cats have four legs and are roughly the same size as dogs, Tiddles and her kind do not deign to fetch sticks that have been thrown for them. Learning about other related categories helps to delineate more precisely the classification being taught.

Connections between categories are thus relevant to the meaning of those categories. Does this mean that we can use these other connections in order to achieve a successful classification of new particulars? If so, we could use the fact that dogs are defined by their possession of paws as a guideline to help us work out if our new item is a dog or not. Finitists argue, however, that although we use our routine intuitions about such connections in these cases, cross-category relations offer no help at all, formally speaking, in making a classification (Barnes, Bloor & Henry, 1996: 52). To decide if a new particular is a dog, we would have to check to see if it has paws. We then come across the same problem as before, because the category 'paws' is constructed from a finite number of instances of resemblance, and the items on the end of our new animal's legs bear both a similarity and a difference to those items categorised under 'paws'. We thus end up with a further question: are they paws or not?

This attempt to connect up with other terms cannot remove the indeterminacy of classification. No matter how many secondary descriptive categories we attempt to apply, we have to make a judgement about the resemblance of members of a category to a new particular (Barnes, Bloor & Henry, 1996: 52). These judgements of resemblance are always

problematic because there is no formal way of measuring whether the similarity of a new case is close enough to established cases to induct it into the category.

So far, the problem of resemblance has resulted in difficulties with classifying new particulars. In a finitist account of matters, the limited (finite) number of instances that constitute a category cannot determine or provide any guidance as to whether a new particular should or should not be included as a member. However, it is not only the case that new objects cannot be definitively claimed to be members of a category on the basis of resemblance. It is also true that *existing* members of a category may be reconsidered and reclassified with no formal constraints, if actors change their mind about the appropriateness of the classification (Barnes, Bloor and Henry, 1996: 57). After all, the resemblance of existing members of a set to one another is just as formally problematic as the set's resemblance to a prospective new member. We are perfectly within our rights to examine existing members of the category 'dog' and decide that some of these members have been misclassified. If a student wishes, they can claim that Chihuahuas are not dogs, and a teacher can give no definitive argument as to why Chihuahuas *must* be classified as dogs. So, not only are future instances of a category problematic, but the current content of a category is formally unstable as well.

It is important to note the generality of the finitist claims. The examples that I have given are from our common-sense learning of animal terms. However, meaning finitism applies to all concepts, including those of a more precise, scientific nature. Thus it is as true of 'carbon' as it is of 'dogs' that new cases of classification are always open-ended, and arguments can be made both ways for whether a new particular should be considered 'carbon' or not⁵. For example, diamond and carbon black are both classified as 'carbon' even though their chemical behaviour is distinct in a way which problematises their classification as 'the same' element (Barnes, Bloor & Henry: 1996: 67). In other words, diamond and carbon black are both similar to and different from each other, and there is nothing in nature or logic which instructs us to categorise them as the same or different. All beliefs or generalisations that we hold are constructed out of concepts of a finite, open character. Just as any new particular may reasonably be included in a specific set, formally speaking, any event or experience may be reasonably incorporated into specific beliefs and generalisations.

⁵ Scientific concepts frequently refer to processes rather than objects, but this causes no problems for the SP approach. Process concepts are learned through ostension and analogy with other inductively produced concepts, whereby initial common-sense experiences are refined and reflected upon to produce the more technical understandings in question (Barnes, Bloor & Henry, 1996: 60-1).

Meaning finitism is largely a negative doctrine. It argues that no act of classification can be definitively justified given a finite set of existing instances and a new particular to be dealt with. Although the SPers support these claims, they do not conclude that the decision to classify one way rather than another is a random one. Rather, they insist that to make classification intelligible we must bring another factor into the equation: the socially defined goals and interests of actors. How, therefore, does reference to the goals and interests of actors bring determinacy to an instance of classification?

Essentially, groups of actors have goals and interests that may be achieved by classifying particulars in one way rather than any of the formally possible alternatives. These interests provide the motivation for the group to push for one kind of classification over another. As a result, alternative classifications that are equally reasonable in a formal sense will not be equally attractive to 'interested' or goal-driven actors. By including reference to social interests, one can make intelligible classificatory decisions that were not explicable by nature or logic alone. Barnes and Bloor's approach is thus 'instrumentalist', emphasising that knowledge is an instrument used by actors in pursuit of their aims and purposes.

A useful example of interested classification is outlined by Barnes, Bloor and Henry in *Scientific Knowledge: A Sociological Analysis* (1996: 121-4). The particular classificatory dilemma involved was whether certain types of red dye, derived from aniline, were to be categorised as 'the same' or 'different'. This became a salient issue during a court case in which the company Renard Freres argued that its patent was being broken by manufacturers selling the 'same substance' as that which it had patented. In accordance with meaning finitism, the SPers argue that, formally speaking, the classification could have gone either way. On the one hand, the dyes of competing manufacturers sometimes had different practical applications, and were distinctly named. On the other hand, calling on a certain chemical tradition, it could be argued that the dyes were identical in their chemical composition, the differences being due to superficial additives not relevant to the essential structure. However, when social interests are at stake in the outcome of the decision, actors have good reason to commit to one classification or other. Unsurprisingly, in this case the members of Renard Freres, its lawyers and supporters held that all the aniline dyes were 'the same', and their opponents, with attendant lawyers and supporters held that they were importantly different. What was undecidable using nature alone became decidable to the groups as they pursued their interests.

From the account of the case given so far, it could be argued that although each side had its own interests to pursue, and thus had clear motivation to classify in one way or another, it

might be possible to appeal to neutral scientists to make a decision in a disinterested fashion. However, Barnes Bloor and Henry argue that this could not have helped to settle matters. Although independent experts have no specific vested interests in the outcome of the case, they have other commitments and interests located in their disciplinary practice. So, it is quite feasible to imagine that a 'realist' chemist from one tradition and a 'positivist' chemist from another might disagree upon the classification of the dyes because of their approaches to classifying substances. In such a case, different kinds of disciplinary practice, involving different aims and interests, would provide the motivation for opposed classifications. Summarising the issue, Barnes, Bloor and Henry argue that there is no way for scientists to be entirely disinterested. They state:

...in the absence of any project at all, in a situation where goals and interests have no role, the question of the sameness or difference of the aniline reds is undecidable and would be experienced as a meaningless one. (Barnes, Bloor and Henry, 1996: 124)

Thus processes of classification and the construction of knowledge can only be properly understood when the motivating goals and interests of actors are taken into account.

This leads us to a further, important aspect of the SP account of interests which must be considered here. Barnes argues that many accounts of knowledge subscribe to a 'Manichean myth' about the nature of the interests involved in knowledge generation (Barnes, 1982: 106-8). When the interest is taken to be oriented to 'prediction and control', this is seen as legitimate, expressing as it does the generalised human need to deal successfully with the environment. On the other hand, when the interest is taken to involve specific ideological or socio-political aims, this is seen as illegitimate, and such interests are held to distort or bias the knowledge produced.

Barnes, however, does not subscribe to this myth. He argues that there is no essential difference between socio-political interests and those related to prediction and control. In other words, socio-political interests are always also to be understood as predictive and oriented to dealing successfully with the environment. Likewise, there can be no issue of 'purely' predictive interests aside from the socially sanctioned concerns about *which* predictions are to be considered important and which are to be discarded as irrelevant.

In order to illustrate these arguments, let us consider two of Barnes' examples. To show the social aspect of predictive interests, Barnes discusses the development of the existing category 'male', on the basis of a new interest or goal that emerges in a subculture (Barnes, 1981a: 326-8). This new goal is to make predictions about the physiological states of the 'males'. In order to achieve this, existing exemplars of 'male' are subjected to empirical

investigation to discover as yet unknown features of this group. However, such investigations may not unproblematically reinforce the pre-existing sense of who should be in the category 'male'. For example, it may be that when investigating 'males', scientists discover that nearly all people categorised as 'male' have the XY chromosome. By altering the category 'male' so that it includes only those who have XY chromosomes, a particular predictive goal is served, and generalisations like 'males secrete testosterone' or 'males grow beards' are made more successful (Barnes, 1981a: 326).

To the SPers, of course, this particular development of the category 'male' is a contingent one. There is no formal reason why 'male' must be developed in a way which emphasises correlations with factors such as the secretion of testosterone and the growing of beards. When a group argues that this is how the category should develop, they do so on the basis of particular social interests that may not be shared by all. For example, there may be other subgroups who believe that developing 'male' in relation to personality or status characteristics is a better option (Barnes, 1981a: 326). Furthermore, each of the possible developments has its own predictive advantages, making a 'purely technical' decision an impossibility. As Barnes states:

Similarity relations can appear in any number of generalisations, and what makes one better to guess with will invariably make some other worse...There is no logic to determine the relative technical advantages of the alternative strategies of concept application: people simply have to agree which generalisations they will take account of, and agree in their practice how they will be taken account of. (Barnes, 1982: 109)

Another example provided by Barnes illustrates the way in which social and political interests influence classification. In this case he discusses the dispute between 'conservative hereditarians' and 'liberal environmentalists' over gender differences (Barnes, 1982: 105-9). He suggests that each group could be said to have interests in sustaining opposed beliefs about the source of gendered characteristics. The hereditarians argue that gendered differences in behaviour, such as degree of aggressiveness, are the result of biological factors. Likewise, environmentalists argue that differences in gendered behaviour are learned, and that such characteristics are not based in nature but society.

According to Barnes, no matter what evidence is produced in the course of this dispute, each party can reasonably continue to defend their own position (Barnes, 1982: 106).

He states:

Conceptual fabrics can always be maintained by Duhem-type strategies so that, whatever their form, they remain both internally consistent and also consistent with experience...Hence the protagonists of hereditarian and environmentalist ideologies could indubitably keep their respective systems consistent in just this way. (Barnes, 1982: 106)

Any evidence that emerges provides just one more new 'particular' to be classified, and, formally speaking, there is no reason why it must be classified in one way rather than another. Classificatory decisions are underdetermined by reason and evidence alone. Instead, groups will classify such evidence in relation to their social and political interests. For example, evidence that shows the variability of male aggressiveness in relation to environmental factors can be construed differently. Environmentalists will claim that it strongly supports their case, whereas hereditarians will argue that it merely illustrates the ways in which innate male aggression can be masked. Each side may be said, then, to be learning from the evidence and making 'reasonable' classifications, without needing to give up their core claims. To Barnes, this suggests that neither group can be interpreted as defending a 'social interest' *instead of* an interest in prediction and control (Barnes, 1982: 108-9). Rather, although divergent classifications serve different social interests, both groups can be said to be 'learning' from the evidence while continuing to defend their central beliefs.

This completes my exposition of the SP account of categorisation. Their account offers answers to the epistemological questions raised at the start of this section, regarding the justification of categories. Barnes and Bloor hope to have demonstrated that categories do not have an internal logic of their own which determines their application. In doing so, they open up space for a sociological account of how categorisations are applied. The next section turns to a critical evaluation of their approach.

4.3 The social and the instrumental

The SP approach to categorisation looks an attractive one, claiming as it does to synthesize a sociological perspective with an instrumentalist orientation. In this section I investigate whether this synthesis is successfully achieved. I argue that, ultimately, the SP analysis fails to incorporate an instrumentalist concern with the success and failure of understandings. In their actual analytical framework, the SPers employ a conception of the 'social' which removes issues of the success and failure of knowledge from consideration. This conception of sociality is the consequence of a dualism between natural science and other social activities, rather than a transcendence of this division. Two consequences ensue. Firstly, Barnes and Bloor's account turns out to be an implausible strong conventionalist position on the nature of knowledge. Secondly, the SP notion of interests turns out to be a problematic one. Although the SPers see the 'social' as 'interested', they cannot explain how the resources to which an interest is oriented are produced. Before considering these arguments however, we must examine the SPers' explicit commitment to instrumentalism.

In many respects, the instrumentalism espoused by Barnes and Bloor is close to the position advocated in Chapter 1. They emphasise that it is not possible to assess the correspondence of knowledge with reality. As Bloor puts it:

At no stage is this correspondence ever perceived, known, or, consequently, put to any use. We never have the independent access to reality that would be necessary if it were to be matched up against our theories. (Bloor, 1976: 40)

Instead, the assessment of a theory is internal, concerned with creating a coherent account of experience and avoiding anomalies (Bloor, 1976: 39).

The SPers also suggest that the theories employed by actors may be more or less successful. As Barnes puts it:

Knowledge arises out of our encounters with reality and is continually subject to feedback-correction from these encounters, as failures of prediction manipulation [sic] and control occur. We seek to eliminate such failures, but so far reality has sustained its capacity to surprise us and dash our expectations. (Barnes, 1977: 10)

In other words, practical success is never guaranteed by our desire to achieve a certain goal: there is always a question of better or worse knowledge⁶. To cite one of Barnes' examples, although cancer research has been strongly supported in the post-war era, the small returns from this research suggest that the will to solve a problem may not easily transform into its solution (Barnes, 1974: 103).

⁶ Bloor concurs with this point, arguing that since our beliefs are created as instruments, 'we must distinguish those which work from those which do not' (Bloor, 1991: 40).

We can see, then, that at the level of stated commitment, the SPers offer a plausible instrumentalist orientation⁷. However, it is important to ask whether their theoretical system of analysis can properly incorporate such a concern. I shall argue, in fact, that this system is set up so that instrumental success, or a lack of it, does not have an explanatory role. The first issue to consider here is whether a social group can achieve an interest or goal whatever experience arises. If a social group sets up a certain classification or knowledge claim in order to pursue an interest of theirs, is it always the case that this classification successfully serves that interest? In other words, can an 'interested' classification be successful no matter what experiences the world throws up for the social group to deal with? In line with the SPers' explicit pronouncements, we should expect the answer to these questions to be 'no'. The desire to reach a particular outcome does not necessarily produce that outcome. With reference to cancer research, just because researchers set up categorisations and classifications to try to predict and control the causes and development of cancer, it does not mean that they successfully manage to do so.

Somewhat surprisingly, when it comes to the SP theoretical system we find that this pragmatic⁸ attitude is dropped. Instead, the SPers argue that any experience whatsoever can be made consistent with an interested classification, and thus any interested classification can always be seen as successful. The clearest statement of this position is to be found in Barnes' *T.S. Kuhn and Social Science* (1982). In this work, Barnes employs Duhemian arguments to suggest that if a social group has an interest in a particular classification or generalisation⁹ they need never find anything in experience that undermines their success (Barnes, 1982: 73-6).

⁷ The main difference between the SP's espoused pragmatism and the critical historicist position defended here is that Barnes views historical knowledge as 'primarily instrumental' (Barnes, 1977: 15). Barnes writes that historians' findings are 'properly thought of as predictions of subsequent archaeological or paleological discoveries; their reconstructions of the past may constitute virtual experiments on the basis of which to learn [sic] how to predict, or even influence, the course of social change' (Barnes, 1977: 15). It seems implausible to describe the aim of historically oriented investigations as predicting the results of other historically oriented investigations! Barnes' use of modifiers such as 'primarily' expresses doubts that a fully instrumental account of historical knowledge can be given, although he offers no suggestion as to what the 'secondary' aspects might be. In the previous chapter I suggested that practical efficacy is a subcategory of wider sense-making activity, and I would argue that historical understanding is a form of sense-making that may or may not have practical consequences.

⁸ For the purposes of this chapter, I use the terms 'pragmatic' and 'instrumentalist' as synonyms.

⁹ For example a generalisation that predicts connections between two categories such as 'geese eat grass' (Barnes, 1982: 74-5).

Talking of the consequences of Duhem's claims, Barnes states:

Not only, as a result, is reality incapable of giving lie to an isolated hypothesis; it is no more capable of indicating the existence of deficiency in a whole set of connected hypotheses. A whole conceptual fabric can always be *made out* as in perfect accord with experience, if the community sustaining it is of a mind to do so. (Barnes, 1982: 75. Author's emphasis)

That is to say, a group can always reasonably claim that experience is confirming their classifications, no matter what actually occurs¹⁰. As such, they can always claim that their knowledge is 'pragmatically successful', whatever the outcome.

A question still remains as to how this can actually be done. After all, we might accept that although all categorisations are 'formally' open, once they are employed for a pragmatic purpose they have a determinacy which cannot be ignored. For example, if we are interested in correlating mobile phone use and developments of a form of cancer, some experience gleaned from research will support this view, with other experience challenging it¹¹. According to Barnes, however, this is not the case. He argues that, when applying an interested classification to new experiences, those phenomena that are found to fit with the category may be inducted into it. Likewise, whatever appears to be anomalous to the existing classification can be placed under a new category, leaving the previous knowledge as it was. Thus, Barnes states:

...whenever anything of nuisance value arises out of experience, it can always be deemed a new kind of thing or event, and assimilated under a new concept, *leaving the existing structure unaltered*. (Barnes, 1982: 75. My emphasis)

To return to the above example, if one wishes to assert that mobile phone use causes cancer, this can always reasonably be done, and any evidence which appears not to 'fit' with these claims can be justifiably placed in another category which does not problematise them¹².

¹⁰ In a debate with Woolgar over the status of interests, Barnes makes a similar remark: 'Almost everyone who accepts the Duhem-Quine hypothesis will recognize that *any* theory can be maintained compatible with *any* findings by appropriate strategies of application and interpretation, and that the strategies involved are just those which maintain our actual accepted theories as our accepted theories' (Barnes, 1981b: 493).

¹¹ Of course, the determinacy here cannot be specified with absolute precision. The precise level of correlation required can be debated in relation to other similar research, and so on. However, to acknowledge a certain amount of flexibility is not to view this as total. Furthermore, the specificity of one's interest gives more determinacy to what counts as 'cancer' and a 'mobile phone' than the total formal openness that exists when no particular concern is involved.

¹² Logically speaking, the converse claim is equally plausible. One can reasonably defend the claim that mobile phone use does not cause cancer without ever running into problems with existing or future experience.

It seems fairly clear that on such an approach, pragmatic success and pragmatic failure are no longer distinguishable. If one cannot differentiate between what is anomalous to a claim and what coheres with it¹³, any claim may be treated as correct (or wrong), and any interest may be said to be successfully (or unsuccessfully) pursued. Pragmatic success does not need to be striven for by investigation: it can be defined by fiat, regardless of what is experienced.

This clearly has important consequences for a sociological approach to knowledge. If the SPers are correct, then an explanation of why actors hold certain beliefs will require no consideration of the practical success or failure of belief. Whatever the interest pursued by actors, this can be said to be fulfilled by their current beliefs and classifications. Furthermore, the pragmatic improvement of a category cannot be a legitimate explanation for changes in a belief, given that there are no grounds for imputing pragmatic failure to the belief at its earlier stage.

I would argue that these claims cannot be sustained. Some of the difficulty with the SP arguments can be demonstrated by returning to one of Barnes' examples, the debate between hereditarians and environmentalists over the causes of sexual difference. Here, I would like to pick up on Barnes' claim that regardless of the evidence brought to bear in this debate, the hereditarians (or the environmentalists) can claim that their theory allows them to successfully deal with the subject matter in question. Barnes asks us to imagine that evidence of environmental influences has emerged in such a way that the hereditarians appear to be on the back foot, desperately trying to protect their thesis against refutation (Barnes, 1982: 109). He suggests, however, that such manoeuvres can equally be characterised as 'learning' for the hereditarians, in which they come to comprehend those environmental factors that can mask innate characteristics such as male aggressiveness. For Barnes, the hereditary theory can always be defended as a successful one, whatever the evidence produced by inquiry.

I would argue, however, that Barnes misdescribes the process of this debate. It is true that in the face of powerful evidence for the environmentalist position, the claim that heredity has *some* influence on behaviour may still be defended. However, this involves shifting from a 'strong' hereditary view in which most or all behaviour is caused by heredity, to a more moderate position that accepts both environmental and hereditary influences. If the evidence pushes hereditarians to accept that some behaviour is influenced by environmental factors,

¹³ See Barnes (1982: 100) for the claim that anomalies are not forced upon theories but are taken to be such for other reasons. Certainly we might argue that the priorities given to resolving anomalies may alter depending on the structure of a research programme and its other successes. However, it cannot be the case that it is optional whether to categorise a certain instance as anomalous to or coherent with an existing theory.

then they can no longer legitimately defend the strong view, regardless of their interest in doing so. Although rhetorically the environmental factors may be described as non-primary, if hereditarians admit that they have to be employed to successfully describe human behaviour, then evidence has produced a shift in the debate¹⁴. In other words, the 'success' of their attempt to describe behaviour as caused mostly by hereditary factors is thrown into doubt.

Even using an example chosen by Barnes we can see that actors may not easily be able to construct knowledge that achieves their aims and purposes. Nevertheless, we need to tackle more directly Barnes' argument that those particulars (or pieces of evidence) that appear anomalous to interested classifications can always be dismissed by not classifying them at all, or by inventing new categories that remove their problematic character. If this argument was indeed adequate, then it could be said that the hereditarians had needlessly capitulated from their 'strong' position. Any group could always reasonably argue that their socially constituted aims and purposes were being successfully achieved.

In order to address this issue, it will be helpful to consider what it means for a piece of knowledge to be 'useful' to an actor. Essentially, it means that certain classifications or generalisations are put together for the purpose of reaching a desired goal. Pragmatic knowledge is thus designed to successfully produce a certain outcome for the actor. This should give us pause for thought in relation to Barnes' argument. It is central to his position that actors can ignore particulars that are anomalous to a category by inventing new categories for them or ignoring them altogether (Barnes, 1982: 75; 1981a: 322). However, the very purpose of the classification is to deal successfully with these kinds of particulars. If it does not do this (as apparent when anomalies emerge) then merely shunting off the anomaly to another category does not help matters in the slightest. In fact, it is precisely a failure to deal with particulars that actors wished to successfully relate to.

I would argue that when the genuinely instrumental concern of action is taken into account, the kind of reclassifications advocated by Barnes cannot make sense to actors. To illustrate this point, we can consider Barnes' discussion of the classification of whales, in 'On the Conventional Character of Knowledge and Cognition' (1981a). Barnes discusses a community who classify creatures into fishes and animals¹⁵ in a similar way to us. However,

¹⁴ Furthermore, the required shift has not been on the 'periphery' of the theory as Barnes suggests, but very much relates to its core premises.

¹⁵ I have followed Barnes' usage here, in which 'fish' and 'animal' are alternative and exclusive classifications. However, it should be noted that fish are actually animals. Barnes' arguments make more sense if we take him to mean 'mammal' instead of 'animal'.

he asks us to imagine that they have never encountered a whale before. When they do so, the question arises as to whether this new particular conflicts with their existing system and forces a reworking of the community's categories. After all, the whale has characteristics split across existing classifications, being 'live-bearing' and 'air-breathing' like animals, but 'finned' and 'sea-dwelling like fish' (Barnes, 1981a: 322).

According to Barnes, the community has a range of options in its classificatory response. They can classify it as a fish, although this could threaten their categorisation of fish, as the whale breathes air; they can classify it as an animal, although this conflicts with generalisations about animals living on land; or, they can designate it as a new type of creature altogether, a 'whale', which is neither fish nor animal. Barnes argues that this last approach is particularly interesting as by creating a new category 'no existing generalizations need be disturbed' (Barnes, 1981a: 322). In other words, an instance that is problematic to existing categories can be made unproblematic by creating a new category for it.

There is something slightly fishy about these arguments. Firstly, it is not the case that a new category has no bearing upon existing classifications. After all, by creating a new category of creature, we alter understandings about how many kinds of creature there are, and how the characteristics of creatures are distinguished. For example, now the categorisation 'sea dwelling creature' will include not only 'fish' but 'whales' as well. Some part of the actors' network of theories is necessarily disrupted by the incorporation of an anomaly, contrary to Barnes' arguments.

More importantly, in line with the SP, we must argue that classifications are made in order to achieve a certain purpose. This means that the categories used to classify creatures exist in relation to generalisations found useful and important by the social group. For example, one difference between fish and animals is their method of breeding, and knowledge of this difference is of interest to humans in relation to their husbandry of resources. Likewise, it may be useful to know the domains of creatures in order to avoid the dangerous ones. Very simply, to avoid sharks, one should stay on the land.

It is therefore in the interests of the actors to put the characteristics of whales into a proper relation to existing categories, so that they can successfully relate to whales as well. To avoid classifying whales, or to put them in a separate category which does not 'disturb' existing categories is to avoid dealing with a whale in a way that is useful to the community. Unless the whale can be fitted in with the understandings of the community it will be unpredictable and difficult to deal with. Although successful classification may require the reconstruction of understanding, it can hardly be in the community's interest not to improve

their understanding of phenomena that impinge upon them. And yet this is precisely what Barnes suggests in relation to anomalous occurrences¹⁶. The strategies suggested by the SPers to avoid problematic phenomena necessarily result in the restriction of actors' knowledge and capacity in relation to their interests. Such strategies would not be pursued by actors, as practical success is a genuine issue for them. In order to understand shifts in hereditarian claims or the way in which a community classifies a whale, analysts must incorporate a concern with the success of the categorisation in relation to the interests involved.

These points are relevant to the more general analysis of classification. We can accept the meaning finitist point that at some general level there are no grounds for validating one categorisation over its competitors. However, when actors have an interest in achieving a certain result, then a categorisation can be demonstrably better or worse at achieving this. It may systematically allow the production of desired effects, or may fail in this task, leaving interactions with the world unpredictable. By denying this, the SPers take the general argument about the indeterminacy of categorisations and apply it to specific socially interested cases. But this is the same fallacy as the arguments for strong underdetermination criticised in Chapter 1. Both claim that a theory can be maintained as consistent no matter what evidence arises. Although this is formally true, what it fails to acknowledge is that without positive reconstruction of categories in the face of anomalous evidence, consistency can only be maintained by strategies that sacrifice resourcefulness. Anomalies can be 'deflected' to save a category. However, it is in the interest of actors to reconstruct their categories so that they facilitate successful interaction with the environment.

Summarising the argument so far, it seems that the SP approach ends up disconnecting social interests from successful categorisation. Initially, social interests were said to be oriented to a concern with prediction and control, that is to say, an interest in successful categorisation. However, the SPers end up suggesting that any categorisation produced by actors can be defended as 'successful' through the employment of strategies to deflect the disruptive power of anomalous instances. I have argued that this misrepresents the process of creating successful categorisations, in which anomalies must be dealt with by

¹⁶ Barnes makes similar moves when discussing how actors relate to earlier classifications. His argument is that these classifications come with no particular goals and interests attached, but are available to actors as a 'resource for those willing to take them as a resource' (Barnes, 1982: 113). I would argue that this is somewhat misguided. After all, those classifications were developed for some purpose, and were more or less successful in achieving it. Any actors who wish to achieve a purpose to which the classification relates can choose to ignore it at the peril of not successfully achieving their goals.

reconstructing knowledge rather than by engaging in defensive strategies which limit understanding. Equally problematically, the SPers end up offering a concept of 'social interests' which is non-instrumental. Groups are held to have an 'interest' in defending a category as 'successful' even when it cannot deal with the particulars that it was intended to predict and control.

Our analysis of the SP system cannot end at this point, however. So far, our attention has focused on issues of successful classification. Now we must turn to consider the nature of 'interests' further. In their explicit statements, the SPers emphasise that an actor's 'interest' in an outcome is an interest in prediction and control. Having an interest involves wishing to control the course of events so that a particular desired outcome or class of outcomes result. Logically speaking, then, the viability of an interest and its continued pursuit by an actor depend upon their ability to achieve this result. The sense in pursuing an interest is related to the success of the understandings which are employed. As we have seen, however, the SPers argue that actors can always be said to 'successfully' achieve their aims, whatever experience throws up in relation to their categorisations. With the success or otherwise of a classification no longer a matter of concern in the SP system, interest statements are not located in relation to the particular modes of prediction and control that make them viable. Instead, they are treated as if they are independent of such concerns. Because the content of categorisations is treated as 'indeterminate', categorisations can be unproblematically manipulated to achieve whatever 'social interest' is being pursued at the time. Nothing within a categorisation can cast into doubt the viability of the 'social interest' being pursued.

A useful example of this style of analysis can be found in Bloor's *Knowledge and Social Imagery*. In this work, Bloor discusses the institutionalised mode of classification whereby the Azande attribute the status of 'witch' to clan members (Bloor, 1991: 138-146). Witches are considered to be trouble-makers by the Azande. They are identifiable by the witchcraft-substance that they have in their stomachs, the existence of which can be confirmed by a post-mortem. Furthermore, this substance is inherited from parents, and any parent bearing it will hand it down to children of the same sex.

Given the nature of witches, it might be inferred that once one witch has been located, this status must also be attributed to all same-sex members of the family line. However, Azande do not do so. When the suggestion was made to them that they should make this inference, the Azande saw the sense in it, but did not accept that this conclusion had to be drawn. Some analysts have suggested that this demonstrates the disinterest of Azande in logic, particularly in a logical inference that might problematise the institution by creating too

many witch attributions. Bloor believes that such analysts are wrong. He states that the apparent logical contradiction inherent in this institution has no force, because it can always be 'negotiated' away (Bloor, 1991: 141). For example, Azande sometimes argue that just because a person has witchcraft substance, it doesn't mean that they are a witch. Rather, witch-hood it is a potential that may or may not be actualised, and only in the former case is a person to be ascribed the status 'witch'¹⁷. As Bloor states: 'Logic poses no threat to the institution of witchcraft, for one piece of logic can always be met by another' (Bloor, 1991: 141).

In Bloor's mind, this illustrates the general point that actors' support for an institutionalised mode of classification has nothing to do with a reasoned analysis of the content of that institution. Arguments about the validity of an institution are always indecisive, and cannot produce a rational conviction one way or the other. Importantly, Bloor argues that it is our commitment to an institution that leads us to defend it as 'reasonable', rather than our belief in the 'reasonable' nature of the institution that makes us committed to it (Bloor, 1991: 143). Likewise, if we experience an argument against an institution as 'forceful', this is not because logical inferences have power, but because we are critical of the institution already (1991: 143).

Although this argument is not couched in terms of interests, it clearly fits with the SP model to 'explain' an actor's commitment to an institution in these terms. Furthermore, it illustrates the way in which the SPers view 'social factors', such as commitment to an institution, as entirely independent from a reasoned and knowledgeable analysis of the institution. Having an interest in defending an institution has nothing to do with reasoned arguments for justifying the institution. Interests and knowledge/reasons are disconnected from one another.

The problem with this separation arises when one tries to specify what it means for an actor to have an 'interest' in an institution. Drawing on the preliminary remarks above, it means that through prediction and control, an institution successfully produces some desired result or benefit for the actor. In order to continue receiving this benefit, the actor supports the institution and its continuation. Likewise, if an actor has an interest in altering institutional arrangements, it is because they believe that their goals will be better served in a new configuration. Locating an interest thus means identifying the structure of the institution, and how it produces and distributes the resources (benefits) which are pursued by actors in their interested activity.

¹⁷ One might thus suggest that Azande are ontological realists.

This means that actors have interests because of the effects or outcomes that an institution can produce for them. However, as we have seen, no outcomes can be achieved unproblematically. An institution may be more or less successful in producing the resources desired by actors, depending on the pragmatic success of the knowledge embodied in it. Importantly, this suggests that debate about the validity of an institution is a reflection upon its practical adequacy for providing benefits to the actors involved. It is not a form of sophistry in which a claim of institutional validity can be defended whatever the evidence, but an argument about the pragmatic success of an institution from the location of particular actors.

Actors' interests and knowledge of the adequacy or validity of an institution are thus intrinsically connected. Actors do not have interests in defending or attacking an institution regardless of its pragmatic success in producing resources for them. Rather, they have interests only because of the success or otherwise of an institution. It is not logical for analysts to hypothesize interests for actors that are not located in relation to an institutional structure of knowledge. Such ascriptions fail to explain how the resources to which that interest is oriented are produced, and thus what knowledgeable activities the institution embodies in order to make a certain interest possible. It cannot be that actors have an 'interest' in supporting or challenging an institution regardless of the success or adequacy of that institution in dealing with the material world.

These problems with the SP account are closely connected to the difficulties that emerge within realism. In the previous chapter we saw that Archer postulated a strategic logic of the social that was not based in the success or otherwise of institutions. Likewise, the SPers argue that conflicts over the validity of an institution are not based in its failure to meet the needs of some actors. In both cases, the existence of social conflict is not properly accounted for, and, instead, floats free of any institutional location and explanation. The difficulty for Barnes and Bloor is that any attempt to provide an explanation for conflict must necessarily consider resources and their generation, bringing issues of success back into a mode of analysis that has ruled them out of consideration.

When interests are reconnected with knowledgeable institutional frameworks, this means that they are subject to alteration and development in the light of developments in

understanding¹⁸. As an example of this we can return once more to the debate between liberal environmentalists and conservative hereditarians. It is possible, as Barnes suggests, that evidence will come in that supports the environmentalist account of behaviour rather than the conservative theory. As this evidence accrues, two shifts may happen. Firstly, conservatives may decide that it is no longer in their interest to defend their social and political resources using a theory which is shown to be weak in its account of human behaviour. If the theory is used to justify existing status and influence, its explanatory paucity may undermine such justifications, and conservatives will wish to look for these elsewhere. They will thus reconceive which claims it is in their interest to investigate and defend; put more strongly, their 'interest' in supporting hereditarianism may have been misconceived given that it undermines their political claims.

Secondly, (some) conservatives may change their political affiliation on the basis that hereditarian claims are not defensible. If the greater adequacy of an environmentalist account has been established, they may come to believe that it is in their interest to participate in a society which actualises the possibilities of all actors and not just those of a certain group. That is to say, they may decide that their 'interest' in maintaining existing status and influence while living in a society which deforms human capacities is less than their 'interest' in living in an egalitarian society with fully realised human beings. Of course, such a serious alteration in political ideology is unlikely to be brought about by a shift in one specific debate! However, a number of such shifts may make a change in 'interest' a plausible outcome.

To take another example, we can consider the case of 'narrow professional vested interests', which are a standard kind of interest referred to in SP accounts (see for example, Barnes, 1982: 114-8, Bloor, forthcoming). A typical account of professional vested interests suggests that scientists seek to defend a particular theory or practice because of the resources of various kinds (status, financial, technical) that accrue to them when this theory is sanctioned by the larger collective. To gain such resources, scientists are taken to have an interest in defending their particular procedures and knowledgeability *regardless* of how successful these procedures actually are. Or, to put it in SP terms, a group of scientists can always give reasons for the claim that their work is 'successful'.

¹⁸ This explicitly breaks with the SP approach, because of its explanatory framework. Given that the SPers argue that all knowledge is 'finite' in character, then interests too would have to be 'finite'. As such, they could no longer be the determining factor which explained how classificatory choices get made. If interests have a 'finite' character, then the decisions made by actors about how to follow them require explanation themselves, as with values and norms (see Barnes, 1995: 53-60; Barnes, 1982: 123-4; Bloor, 1991: 172-3).

In order to respond to such an account, it is necessary to reiterate that procedures and the knowledge that they embody cannot always be made to appear successful at dealing with some subject matter. When comparing competing approaches, it is often possible to decide which is better at handling the phenomena in question. When a theory has to resort to the kind of protectionism advocated by the SP, it is necessarily limiting the scope of its application, and competing theories may exist that can handle the anomalies that it does not deal with. In such cases, judgements about relative success can be made.

Of course, scientists within a research group wish to present their own approach in the best possible light, so that they can gain the resources at stake in the scientific enterprise. However, it will not be in their long-term interest to continue defending a procedure or theory that is manifestly less successful for than that of its rivals. In such a case, the 'interest' in defending a theory diminishes, even when it is a theory that has well-established successes behind it. When resources come to be allocated to more practically successful theories, there is no longer an 'interest' in defending the theory in question. This kind of shift is part of the dynamic of science.

Such points bring us on to territory explored by Andrew Pickering in *The Mangle of Practice* (1995). Pickering emphasises that there can be no such thing as a 'purely' social interest which stands outside of the practical success or otherwise of scientists' interactions with the material world. He argues that goals and interests are reconfigured as investigation continues, and are interrelated to the outcome of scientific work and its more or less successful character. Pickering then uses these arguments to criticise the SPers for their lack of concern with how practical activity can generate a change in interests. He states:

The tendency is to write as if the substantive interests of actors were present and identifiable in advance of particular passages of practice, setting them in motion and structuring outcomes without being themselves at stake. (Pickering, 1995: 64)

Bloor has responded to these arguments, suggesting that they misrepresent the SP position. To show that the SP can deal with the practical and reconstructible nature of interests, he considers how we might analyse the development of a scientific paradigm (Bloor, forthcoming). Bloor asks us to imagine that this paradigm is becoming 'progressively consolidated'. When this occurs, it is likely that a divergence of interest will result between those who founded the paradigm, and those who joined later in order to develop and articulate its details. The founders will have an interest in making sure that their work continues to be used in the development of the paradigm. Newer recruits, however, will see their interests as being served by breaking the existing paradigm in order to search

for a radical new approach, of which they would be recognised as founders. The consolidation of a paradigm thus results in a shift in interests for those involved.

Bloor is correct to assert that this kind of SP analysis is a dynamic one, in that it does not rely on social interests remaining unchanged. However, the important issue here is not simply whether 'interests' change or not, but whether these changes are adequately theorised. Bloor's example, in keeping with the SP approach, analyses shifts in 'interests' as if they could be located without consideration of the adequacy of the scientific activity they are bound up with. Questions of adequacy are relevant, however, to understanding the interests of the actors involved. If, for example, the development of a paradigm along traditional lines continues to provide new insights and illumination, then it is in scientists' interests to keep working along these lines. Likewise, if it appears that such lines are being exhausted, it is in scientists' interests to break away from the tradition. Without an analysis of the paradigm's success, however, such interests cannot be correctly identified. The implication of Bloor's analysis is that it is always in the interests of actors to be 'founders' of a paradigm. This seems fundamentally implausible, and implies that novel analysis will always be rewarded by the scientific community, no matter how successful it is in accounting for phenomena. Surely it is more likely that credit is given to *successful* new developments, whether this involves founding a paradigm or developing it. Bloor's claim that later generations have an interest in rejecting a paradigm is only defensible in cases where a paradigm's insights are close to exhaustion and a successful alternative can be developed. However, neither of these factors can be incorporated into the SP account.

In this section I have argued that the conception of 'social interests' employed by the SPers is unpragmatic in two important respects. Firstly, they argue that the knowledge produced in order to achieve a 'social interest' can always be characterised as 'successful' no matter what experiences arise. This wrongly suggests that actors and analysts cannot make pragmatically defensible discriminations between knowledge that is more or less successful for a task. It also negatively characterises 'social interests' as interests in defending knowledge that is unsuccessful, that is, knowledge that has anomalies. Secondly, they do not locate 'social interests' in the practical success (or otherwise) of the knowledgeable structures of activity in which actors participate. As such, statements of 'social interest' become inexplicable in pragmatic terms, as do conflicts of interest. Furthermore, considerations that 'social interests' may change and develop through reflection and the development of knowledge are ignored. The overall outcome is that the SP theoretical

system reverts to a notion of 'the social' which is indeed antithetical to interests in prediction and control.

Two quite different conclusions can be drawn from the criticisms of the SP presented here. The internal contradictions within the programme could be taken as further evidence for the illegitimacy of constructing a fully sociological account of knowledge. The arguments offered here could then be added to those put forward by many other critics of social constructionist accounts of science, who see the failure of the SP as a demonstration that a completely 'externalist' account of the content of scientific knowledge is not possible (see, for instance, Bunge, 1991; Slezak, 1994). I would argue that a different conclusion can be drawn, however. The SP argument is that a sociological account of knowledge should be a instrumentalist one, emphasising that social interests must be seen as interests in reaching certain outcomes through successful prediction and control. I suggested that this seems to be a plausible way of interpreting interests which coheres with a defensible philosophy of science. The problem with the SP is not, therefore, its attempt to construct a social account of knowledge, but its failure to see this project through properly. As we saw, instead of social interests being intelligibly linked to prediction and control, they are used to account for commitments to knowledge that cannot be justified in those terms. Identifying a commitment as 'social' is thus tantamount to indicating its anti-pragmatic orientation, much as Barnes and Bloor try to imply otherwise. Having a social interest in knowledge comes to mean defending this knowledge even when it is against an actor's interest in prediction, control, and the achievement of desired outcomes to do so.

There is no need to treat social interests in this fashion. Contrary to anti-sociological philosophers, if we take a social interest to be a concern with the production of resourceful outcomes, then not only is this a legitimate input into science, but it is the very stuff from which science is made. Such interests can be driven either by narrow professional concerns or wider socio-political goals. Contrary to constructionist sociologists, it cannot be assumed that the process of investigation will not affect the validity of these interests. Having an initial interest in producing a result does not mean that such a result will be viable. What investigation shows to be possible may well transform a group's interests, either by showing them the difficulties with achieving their existing goals, or by suggesting new possibilities that would result in better outcomes for the group. Both of these aspects are present in the example of the hereditarians, in which processes of investigation show their claims to be hard to defend and suggest a different way of conceiving what course of action is to their benefit. Scientific investigation is thus a process of transformation of interests rather than

simply a reflection of the interests that existed to begin with. It is not unique in this respect, and all social practices share its character as oriented to achieving some outcome. What is impressive about natural science is how successful it is in investigating how outcomes might be reached, and in transforming our sense of what outcomes are possible in our interactions with the material environment. Nevertheless, this is not a reason to see it as qualitatively different to other social activities.

4.4 Social institutions and success

Up to this point I have been considering the SP attempt to analyse natural scientific investigation as a fully social activity. I have suggested that this attempt fails because the conception of sociality employed is one which cannot account for the variable success of interested belief. Instead of transcending the natural science/social activity dualism, Barnes and Bloor attempt to analyse the former using an unreconstructed conception of the latter. However, this addresses only part of the problem with the SP position. The argument of this thesis is that natural scientific investigation is an activity that has the same characteristics as other social practices. If this is the case, then the SP approach must be just as problematic when applied to other social institutions as it is when applied to natural science. In other words, the failure to consider the variable success of institutionalised activity must problematise analysis of other social activities in the same way as it does the analysis of natural science. In order to see the links between the SPer's analysis of science and other social practices, we must move on from theories of classification, and consider their account of social institutions. This theory unites the analysis of natural science and other practices, and we shall consider its application to both in order to show that the same problems arise in both cases.

The account of social institutions developed by Barnes and Bloor is a general theory of how sociologically relevant phenomena are to be understood¹⁹. It provides a basic characterisation of the institutionalised modes of knowing and acting which are constitutive of social life. Whenever we see a regularised mode of interaction undertaken by human beings, we can consider this to be a 'social institution' in which the shared understandings that make the interaction possible are 'social constructs'. On the SP account, the key

¹⁹ The theory of institutions is based in Barnes' paper 'Social Life as Bootstrapped Induction' (1983), and is discussed further in his book *The Nature of Power* (1988). Bloor has been responsible for more recent developments, particularly in relation to the sociology of natural scientific knowledge (see Bloor, 1996; Bloor, 1997a; Bloor, 1997b). Martin Kusch has also elaborated on the theory, and applied it to debates around psychological knowledge (Kusch, 1999).

features of social institutions are that they are self-referring and self-validating. To say that an institution is self-referring is to say that it only exists because people believe and act as if it exists. It is solely because of the beliefs and actions of a social group that an institution has any reality whatsoever (Bloor, 1997b; Barnes 1988). Although such a conception seems obscure, some examples may help to make it clearer. One important social institution is money. Money has no existence except when actors believe that it is money²⁰. As Bloor puts it:

Money is an institution. Now, something is money, and can only operate as money, only if its users treat it as money, and think of it as money. We can sum this up by saying: money is money only if it is *called* money. (Bloor, 1997b: 12. Author's emphasis)

Another example that Bloor uses is that of leadership. Someone is only the leader of a group if they are taken to be the leader by group members. It is a construction that applies to a person if the members of a group believe and act as if it applies. Otherwise, it has no purchase or existence (Bloor, 1997b: 13). To call an institution self-referential, then, is to say that it owes its character purely to the belief of those actors who create it. Only by their 'reference' to the institution does it have a form and existence.

The SPers also argue that if an institution is self-referring it must be self-validating (Barnes, 1988: 46; Bloor, 1997a: 38). An institution is only constituted by the claims about it made by actors. Therefore, suggest the SPers, these claims are fully constitutive of its character. Given that an institution is created by actors' beliefs about it, it can have no character other than that ascribed by those beliefs. If, for example, a social group treat a person as a 'leader' then this person is validly described as a 'leader'. As Bloor suggests, analysing a social institution in this way treats it as an 'actor's category' in which the actor's beliefs are the final word²¹. Nothing can be gained by close scrutiny of a social institution in an attempt to understand it better than the actors involved (Bloor, 1997a: 33-5). Actors' understandings are valid understandings.

These arguments run directly parallel to Giddens' claims about 'mutual knowledge' that were considered in Chapter 2. Giddens suggests that in order to understand actors' beliefs,

²⁰ Of course the material substances such as paper and discs would still exist if nobody believed they were money, but they would not exist as *money* (Bloor, 1997b: 4).

²¹ In an odd footnote, Bloor argues that the notion of an 'actor's category' is not an actor's category. Instead he says that 'It is part of a perspective which is beginning to go beyond that of actors themselves and represents a new level of self-awareness' (Bloor, 1997a: 150, fn6). Presumably the 'self' that becomes 'aware' is also an actor, and thus the categories that this self uses are subject to the standard SP analysis regarding their self-referential character. Certainly, further elucidation is required by Bloor to explain how why his own analysis is not a form of action.

analysts must treat them as valid on their own terms. When beliefs are apprehended in this way, no criticism of them is justified. However, there is a difference between Giddens' approach and that of the SP. For Giddens, natural scientific beliefs do not need to be treated as mutual knowledge in order to be understood. For the SPers, conversely, all understandings, including those of natural science, should be considered as social constructs and thus self-referential in character²² (Bloor, 1997b). Bloor contends that the social institution model is the *only* possible way to account for the meaning of natural scientific concepts and beliefs. His argument goes as follows. Imagine that we are trying to understand how actors classify a particular and thus create a piece of knowledge. If we exclude social processes, all we have are individuals coming to independent judgements about the similarity or difference of this particular to other particulars. The question is, can an individual's judgement of similarity or difference be considered right or wrong?

An individual making judgements will have to rely on their innate biological and psychological capacities (Bloor, 1996). Put simply, they have 'pattern matching' capabilities which can be used to discriminate between particulars based on the input received through the senses. The activities of such pattern-matching machinery will produce an output which indicates whether a new particular fits a category or not, depending on whether the pattern matches or not. However, there is a problem with using such outputs as the basis for discriminating right and wrong judgements. It is always possible that the machinery will function inconsistently, classifying the same inputs in different ways due to malfunctions or shifts in the mechanisms. Unfortunately, there is no way for the lone individual to detect these changes, as the machinery provides the only discriminatory capacity that they have. As Bloor puts it:

The pattern-matching process has no measure outside of itself to determine whether its matching is going well or ill. There is no standard of a right match or a wrong match. Whatever seems right to the pattern matching machine is right, which means that the word 'right' has no real application here. (Bloor, 1996: 847).

²² Barnes and Bloor seem to view this issue differently. Much of Bloor's recent work has relied on the notion that natural scientific knowledge is self-referential. However, some of Barnes' remarks are more equivocal, particularly in *The Nature of Power*. For example, at one point Barnes suggests that '...knowledge of nature may be confirmed or disconfirmed by processes involving reference to states of affairs that exist independently of the knowledge...' (Barnes, 1988: 52). Two pages earlier, Barnes expresses a seemingly contrasting view suggesting that 'Natural order is just as much a system of conventions as social order' (Barnes, 1988: 50). Given that, for the SPers, a convention is self-referring and self-validating, it is hard to see how it can also be confirmed or disconfirmed by an independent state of affairs. It seems to me that seeing natural scientific concepts as self-referring coheres better with the general SP approach than the alternative.

In other words, because of their reliance on a mechanism which they cannot check or standardise, individuals cannot, on their own, make a 'right' or 'wrong' judgement. The judgements that they make are subjective.

Bloor then argues that for there to be a genuine standard or knowledge claim, there must be factors involved that are external to the individual. This is where sociological analysis comes in to the picture. Unlike an individual, a social group can create a genuine standard for correct and incorrect classification. The standard will provide grounds for asserting that a classificatory judgement is right or wrong, in a way that transcends the individual, and represents the judgement of the collective (Bloor, 1996). It is this standard which determines which particulars are correctly classified under a concept and which are not. Of course, given its social character, this standard is a self-referential institution. It is constituted solely by actors' references to it in their classificatory practices, and its reality and character consists only in these uses (Bloor, 1996: 848). As Bloor puts it:

Just as money is created by self-referential processes, and just as leadership is constituted by self-reference, so the standards of right and wrong concept application, and hence the *content of concepts*, is similarly constructed. The conclusion could be expressed by saying that concepts with an empirical reference are, and must be, social institutions. (Bloor, 1997b: 15. Author's emphasis)

Clearly, this is a strong claim, and Bloor is aware that it may appear to locate scientific knowledge purely in the realm of the social, rather than grounding it in physical reality. He then suggests, however, that this would be a misinterpretation. His approach is intended to incorporate *both* natural and social components as necessary aspects of an account of scientific knowledge. As we have seen, the social component provides the meaning and content of scientific concepts. Nevertheless, such meaning is connected to the physical realm, in that the particulars to be classified are identified through their causal impact upon actors, which involves both the causal powers of objects and the biological capacities of individuals.

Bloor illustrates his approach by analysing Marie Curie's discovery of radium (Bloor, 1997b). He poses the question: is a sociological analyst required to say that instead of 'discovering' radium, Curie 'constructed' it? Bloor's reply is to emphasise that Curie and her fellow researchers did not 'construct' the actual substance radium²³ – they discovered it. However, something was socially constructed in the process – the natural scientific concept 'radium'. Curie developed and reconstructed the concepts of the scientific community in the

²³ Of course the researchers did work physically on the substance with various devices, but did not 'construct' it out of social processes by referring to it.

course of her investigation. Her resultant understanding of 'radium' is a social institution and the analyst can examine 'how it is used to build up the idea of the object thought to correspond to it' (Bloor, 1997b: 19). Bloor's analysis of this episode thus retains both the material character of the substance radium and the social character of the scientific concept 'radium'.

The difficulty with relating the material and the social in this way is brought out by Latour, in a recent debate with Bloor. Latour asks how, in the SP account, the material world that we encounter makes a difference to the content of the scientific concepts that we develop to understand it. It seems that, for Bloor, the world's impact upon our concepts is minimal (Latour, 1999b: 117). Certainly, as Bloor argues in his reply, the causal forces of the material world must provide the stimulus that leads actors to classify them (Bloor, 1999b: 134). But what influence upon the categorisation do they have apart from this? In order to conform to the SP account, the answer must be 'none'. This is because, for the SPers, the content of scientific concepts is self-referential and self-validating. If indeed causal inputs were to have an impact upon the meaning of the concepts developed to classify them, then these classifications would no longer be self-referential. At least part of the content of the classification would be determined or influenced by something other than the concept which the social actors saw fit to bestow upon it. Latour argues that because of this stipulation, the SPers end up treating particulars and sensory inputs as 'tokens' which bear whatever classification is socially and self-referentially determined by actors²⁴ (Latour, 1999b: 117-8).

This criticism is borne out by Bloor's account of the discovery of radium. He argues that a sociological investigation of the concept 'radium' cannot proceed by considering the adequacy of the concept in relation to the substance radium. Rather, to employ Bloor's words once again, sociology focuses 'on the concept itself and on how it is used to build up the idea of the object thought to correspond to it' (Bloor, 1997b: 19). Sociological analysis must not treat the substance radium as if it could influence the 'concept' which describes it. Instead sociology should treat the concept as a social institution, self-referentially constituted

²⁴ The force of this claim is illustrated by the fact that both money and scientific concepts are held to be self-referential institutions. In the case of money, the meanings are socially constructed, and the discs and papers are mere tokens bearing that meaning. In the case of scientific understandings, the concepts are socially constructed, and the particulars or sensory inputs are mere tokens bearing those concepts.

by the actors who create it. Therefore, the substance itself is only relevant to an account of the concept insofar as it is a token bearing socially generated understanding²⁵.

This outcome directly parallels the meaning finitist approach discussed in the previous section. It suggests that actors can categorise particulars in any way, and that such categorisations can always be defended as reasonable on their own terms, as they are self-validating. When we have grasped how actors classify, there is no further task for the sociologist in considering the success or failure of the classification. Furthermore, explanation of how classifications change and develop cannot call on the intrinsic characteristics of the classification itself, but must be accounted for using extrinsic 'social factors' such as interests. Once again, this cuts an analysis of social interests loose from the success or otherwise of the mode of activity engaged in to realise them.

An alternative approach is to take the pragmatic character of classification more seriously. In particular, this means questioning the idea that categorisations are self-validating. On a pragmatic account, classifications are created in order to help deal successfully with the world. However, if all classifications were self-validating, there would never be a genuine issue of success or failure in relation to them. Every classification could be said to be successful for its intended purpose, so long as the community believed that it was successful. Given that a social institution's content is exhausted by actors' descriptions of it, there could be no issue of further investigation to decide if it *really was* an adequate classification²⁶.

Contrary to this, I would suggest that the adequacy of classifications can always be questioned on pragmatic grounds. It is precisely the issue of pragmatic success which allows a social group to inquire into the adequacy of their understandings, and which suggests that understandings should not be taken as self-validating. In fact, our understandings are

²⁵ Bloor attempts to make these claims seem plausible by separating out the interests of a sociological analyst from those of a scientist, suggesting that their interests are different and non-contradictory (Bloor, 1997b: 10-11). A scientist is interested in how a concept stands to 'reality', and how well it describes this reality. A sociologist, on the other hand, is interested in how a concept has come to have its content through its construction by social processes. Contrary to Bloor, it is fairly clear that these interests are not different from each other. In effect, both scientific actors and analysts have an 'interest' in trying to explain how the content of the concept 'radium' emerged. Scientists, however, try to explain this by claiming its contents is a result of a (more or less) thorough investigation of reality, which the concept (more or less) thoroughly describes. Conversely, SP sociologists try to explain this content by its emergence through self-referential social processes. These explanations are competing: if one is correct the other cannot be. There is no divergence of 'interest' as Bloor suggests, but a divergence of substantive claims, which cannot be treated as complementary. and

²⁶ This point is echoed in Barnes' discussion of rule-following where he states: 'When the collective applies a rule in a given way, carefully and honestly, it is unclear how a sense of incorrectness can be generated with respect to that application at that time (although the collective may well *later* change its practice and hence its evaluation of what it had earlier done)' (Barnes, 1994: 28-9). Quite why such a shift would occur is not apparent, especially given the self-validating and thus unproblematic character of the earlier rule-following.

constantly undermined by the problems that emerge when we apply them to some subject matter in order to achieve prediction and control. These problems necessitate the reconstruction of our concepts so that we can increase our capacity for successful action.

If we take the example of radium, we should certainly accept that the concept of 'radium' is a human construction, and one which cannot be directly compared to radium 'itself' in order to assess its adequacy. However, we can ask how well this concept allows us to interact with physical reality. Bloor describes how the new experimental results produced by Curie led to changes in the existing corpus of understanding, with earlier concepts being reconstructed and new concepts being developed (Bloor, 1997b: 17). We are entitled to ask why, if understandings are self-validating, such a reconstruction had to occur. On that view, the 'new properties' displayed by radium could have been unproblematically incorporated into those of an existing element category such as uranium, without the need to create the new category of 'radium'. Surely it is more plausible to argue that the evidence problematised existing categorisations and necessitated a new category which offered a more coherent theory of the elements and their nature.

The general point is that because of their purposive nature, classifications contain implicit or explicit criteria for their successful application. When these classifications are applied in practice they frequently fail to meet such criteria, requiring them to be reconstructed in order to be applied more successfully. In other words, theories generate residual categories containing anomalous outcomes, which necessitate the reconstruction of the theory in order to achieve success. Because beliefs can fail to live up to their requirements, it is wrong to view knowledge as self-validating. To create a classification in order to achieve a purpose is not the same as having a classification which actually achieves that purpose. It is in this sense that analysts can do more in relation to actors' understandings than merely grasping their content. Such understandings can also be scrutinised as to whether they successfully achieve the results which are intended by actors.

Of course, it could be argued that actors sometimes have an interest which limits their concern with phenomena that do not fit with their classifications. Certainly, understandings may be useful in relation to particular purposes which can be achieved to the technical requirements of actors involved, even if residual categories are produced. Nevertheless, two further points should be noted. If a theory emerges which produces a greater systematic capacity and removes earlier residual categories, the technicians would be able to recognise the superior reach of the theory, even if it was of little interest in relation to their concerns. It would not be the case that their theory was 'equally valid' despite its anomalies. Secondly,

we should not reify the purposes of actors at some particular time, as these purposes are possibilities intrinsically connected with the knowledge available. Thus, when knowledge develops, it is possible that people's sense of the important 'purposes' to pursue will also develop in relation to the increased capacity. Instead of seeing 'goals' and 'interests' as fixed boundaries upon the development of knowledge, we should see them as evolving along with this development.

Having discussed the problems of analysing natural scientific knowledge as a social institution, we must now turn to consider whether the social institution model can deal with other social processes. Frequently, constructionist arguments are held to be implausible in relation to natural science, because of the independence of the object from the knowledge about it. Such a perception may, however, differ when considering those 'social' practices and institutional modes of behaviour which seem to lack an independent referent. However, to accept a distinction between the two is to invoke an untenable dualism of natural science and social activity. Non-scientific social institutions are no more self-referring and self-validating than those of natural science. When considering the adequacy of natural scientific knowledge, I argued that the grounds for critique and development could be found in the failure of classifications on their own terms. That is to say that categorisations can be criticised for failing to meet the goals that they are supposed to fulfil. In order to critically assess other social institutions we need no further consideration than this. Institutions can be analysed in relation to the purposes and goals that they are supposed to facilitate, and one can theorise the extent to which they achieve their purpose.

Perhaps the most persuasive of the SP examples is that of money as a self-referential and self-validating social institution. The SPers argue that money is only money because actors take it to be so, referring and acting on the nature of this belief. As Bloor puts it, 'It is not an exaggeration...to insist that if we abandoned references to money, money would indeed vanish' (Bloor, 1997a: 30). Money persists as an institution because people believe it will persist; each time this belief is put into action, it helps to sustain the institution of money further (Barnes, 1988: 83). Money's past reliability convinces actors that it is trustworthy, and in treating it as such, they sustain its trustworthy character.

However, if we attempt to locate the institution of money a little more carefully, it becomes doubtful whether it can be considered self-referring and self-validating. In the first place, it should be emphasised that the institution of 'money' is not an aesthetic creation of actors intended to embroider their culture with yet another self-referential pattern of

activity²⁷. Rather, it emerges as an attempted solution to certain difficulties in the realm of exchange, and it is intended to resolve these difficulties in a productive and sustainable manner. In other words, actors support and use the institution of money because it helps them to realise certain purposes²⁸. As is commonly noted in monetary theory, there are various functions that money can fulfil, including operation as a store of value, means of payment, unit of account and unit of exchange. When it fulfils these functions successfully, money is a central part of an economic system, contributing to the production and distribution of the goods produced therein. As Victor Morgan notes, the development of money as a medium of exchange was an important factor in the development of a division of labour, facilitating the ease of trade required to make specialisation viable (Morgan, 1965).

To see the problems of viewing money as a self-referential institution it will be useful to consider one of the functions that money serves within capitalism. If money is to contribute to trade, it must act as a reliable means of exchange whereby it retains its purchasing power in the periods between exchange. That is to say, the money accepted for one kind of good should retain its value so that a market equivalent good can be purchased using the same amount of money. When a monetary system achieves this, it provides a generalised medium of exchange which successfully facilitates the production and trade of goods.

I would argue that the self-referential picture of money cannot account for money's successful operation because it excludes consideration of the external conditions to which actors' belief in the institution of money is intimately related. The first such condition is that the money supply must be limited for money to function properly. With no such limits, money stops being a successful medium of exchange, and large variations in supply impact upon exchanges being made, to the extent that actors cannot assume that the money they gain from a sale will purchase the equivalent of what they sold. It should be emphasised that such variations in the money supply may have an impact upon exchange and the value of money even though no actor understands the causal factor at work. That is to say, it is not just that agents must 'trust' that the money supply is limited, as Barnes argues (Barnes, 1988: 184, fn 29). This is a claim that would add the need for self-referential confidence in issuing agencies to the self-referential confidence in money. Rather, no one may actually

²⁷ Bloor's account in *Wittgenstein, Rules and Institutions* comes close to this position. He states that the development of money 'is a striking exercise in pure creativity' continuing that 'A group of people have brought something into existence simply by thinking about it and talking about it' (Bloor, 1997a: 29). To be fair to Bloor, his account of the institution of 'promising' later in the work has a more concrete historical dimension.

²⁸ This is not to say that money is the only institution that could serve such purposes, and Jacques Melitz discusses how sophisticated bartering arrangements fulfil some of the same functions of money in certain social structures (Melitz, 1974).

comprehend that a large increase in the money supply will in fact undermine confidence in money as a medium of exchange²⁹. The problems generated by such an increase are not created because actors lose 'trust' in the issuing agency, but because an excess amount of money drives up prices. This leads to problems of exchange not based in a lack of self-referential 'belief' in money, but the failure of institutions to regulate the money supply so that successful exchange is facilitated.

It is just as important to emphasise that confidence in 'money' is not a purely internal, self-validating phenomenon but closely related to the success of economic production. This becomes particularly salient when governments no longer underwrite the value of money by guaranteeing convertibility into an intrinsically valuable commodity such as gold. As Parsons argues in his discussion of power, continuing faith in the value of money is then related to the practical economic success with which money is bound up (Parsons, 1967: 334-5). If the money supply expands with an increase in economic productivity, then actors can be confident that money will successfully facilitate their economic transactions. Likewise, if the money supply outpaces productivity increases, the result is inflation and problems with the functioning of money, which may be more or less severe (Parsons, 1967: 335). It is therefore successful economic processes that provide the real value of money, as it is economic resources that money can ultimately be used to acquire. Confidence is thus based in the practical success of the economic institutions. Interestingly, Barnes misses this connection of resourcefulness and confidence when he reconstructs Parsons' argument (Barnes, 1988: 14-9). He notes that money must, for Parsons, operate on the basis of institutionalised confidence but does not link this confidence to productivity.

The key to this issue is not to deny that there are issues of confidence in the institution of money, but to properly locate this confidence. The *successful* functioning of the institution is produced by the management of money in relation to other institutions, both those issuing and controlling money, and those relating to the production and distribution of goods. A consideration of money should focus upon the conditions required to make money work, and to continually engender the confidence of actors³⁰. By viewing institutions such as money as

²⁹ The inflationary pressures of increased money supplies (caused by an influx of precious metals into an economy) were misunderstood in medieval times, and price increases were blamed upon the 'malpractices of traders' (Morgan, 1965: 181).

³⁰ This returns us to a view of cycles of confidence more akin to that of Merton, who emphasises that institutional management may stop or limit self-destructive spirals of belief (Merton, 1957). There is a certain irony here, given that Barnes' work on self-reference and self-fulfilling prophecies is inspired by Merton's approach (Barnes, 1983).

self-validating, the SPers ignore the complex measures required to keep it (and processes of production and exchange) functioning at all.

Furthermore, the SP account cannot explain how an institution that is successfully set in motion ever comes to change. The implication must surely be that once a self-reinforcing circle is brought about, it has an unstoppable logic of its own, its reliability in the past producing its reliability in the future. Of course, institutions do change in character. The operation of money has altered over time with the gold standard being introduced and abandoned, the expansion of credit, and so on. Particular monetary systems have also changed in status, being successful at some point and then failing. Countries which have been subject to hyper-inflation may abandon their currency and start anew. It is hard to see how any institution that is self-validating would ever be undermined in this way.

In order to address this kind of issue, Barnes resorts to an explanation based on factors 'external' to the institution. Thus, in the parallel case of the success or failure of a bank, Barnes suggests that contingent events may begin to undermine a bank, such as 'inexpedient economic policies' or 'military adventures' (Barnes, 1983: 538). But this is to insist that confidence in the bank is not self-validating after all. If confidence in a bank truly is self-validating, no one need pay any attention to other factors. As with natural scientific knowledge, 'Duhem-type strategies' can always be employed to show that the institution is functioning successfully. Any switch in belief from 'confidence' to 'lack of confidence' would be inexplicable, given the supposedly self-sustaining nature of confidence. To explain change, Barnes has to resort to types of factors which undermine the consistency of the SP framework. In many ways, this difficulty is the same as that encountered in the SP treatment of 'radium'; that is to say, the (supposedly) self-validating character of money makes changes to the institution as inexplicable as the changes made to (supposedly) self-validating chemical theories in order to incorporate Curie's experimental results. It is more appropriate to say that just as problems with the functioning of 'money' may lead to institutional reform, problems with the functioning of existing chemical concepts may necessitate their reform. Neither should be treated as self-validating.

The problems with the SP approach are similar to those arising in Giddens' work (considered in Chapter 2). Giddens suggests that in order to understand actors' beliefs they must be treated as self-validating (i.e. as mutual knowledge). He also argues that once social beliefs are understood, they can be subjected to empirical criticism. However, if actors' beliefs could be correctly understood as completely valid knowledge, there could be no grounds for criticising them. By committing to the idea that actors' beliefs are, at some

level, fully satisfactory, both Giddens and the SPers run into difficulties. For Giddens, the difficulty is finding a role for criticism, whereas for Barnes and Bloor, the problem is explaining change.

Another illustration of an SP analysis of social institutions can be found in Barnes' discussion of status groups and class analysis (Barnes, 1995). Barnes wishes to argue that the study of many social organisations can be enhanced by analysing them as status groups. Importantly, this includes redescribing 'classes' as 'status groups'. So how does Barnes characterise these different types of collectives? For Barnes, a status group is a collective that attempts to monopolize resources such as goods and opportunities (Barnes, 1995: 130-1). This monopolization is to be effected by setting up boundaries that divide social actors into members and non-members, and then pursuing strategies to maximise the advantage of the former. A key feature of such groups is that their constitution is in some sense arbitrary. That is to say, there is no underlying structural logic which makes the emergence of a particular status group intelligible (Barnes, 1995: 143). Rather, argues Barnes, an 'innumerable' range of groups might have emerged, each one of which could have generated a profit if it had successfully enforced a monopoly. It is only once certain groups actually start coalescing that structured interests emerge for actors to pursue, rather than such groups forming around pre-structured interests³¹ (Barnes, 1995: 143).

On this characterisation, we can see that a 'status group' is envisioned as a self-referential, self-validating phenomenon. The interests associated with such a group are not based in some 'objective' or 'external' feature of the structural position of their members. Rather, these interests are generated by the self-referring beliefs of actors that they constitute a group with certain interests. It is only through acting on this belief that such interests genuinely emerge, through the processes of monopolization and exclusion which are subsequently pursued.

Although Barnes offers various criticisms of 'class' analysis, it is arguable that his key reason for rejecting it is that it does not conform with the social institutional model. Class theories postulate that because individuals share a common economic situation, they have a disposition to organise and act together in order to pursue the interests based in this situation. Such action will be an attempt to further the objectively given interests of the group (Barnes, 1995: 173). However, such analysis is contrary to Barnes' argument that social institutions

³¹ Of course, this does not mean that no conditions must be met for a status group to form as a collective actor. Status groups are typically based on an existing interaction network that is somewhat distinct from that of other groups, as such distinction helps to generate a sense of boundaries between group members and others. There must also be mechanisms for reinforcing group membership and maintaining this sense of distinction.

are self-referential and self-validating. If there are real, knowable interests given in social structure, then these interests exist despite actors' understandings: it could be the case that actors believe themselves to have one interest when, in fact, they have another. Actors' beliefs would not be self-validating, but instead would be potentially sub-optimal in relation to their possibilities.

For Barnes, then, class analysis misconceives the nature of interests. There can be no structural statements of the interests that actors have despite their knowledge. Interests can only emerge when social interaction leads to organisation, and such interests will be 'locally defined' by the sanction of a 'specific interacting collective' (Barnes, 1995: 182). Certainly, groups may organise around economic issues, but they do so through self-referring constructions of interests, not in relation to objective social positions. Interests are the product of collective belief, and nothing else.

Barnes' arguments are not without their problems. We can start off by considering that although status group analysis focuses on the monopolisation of resources in their distribution, these resources also have to be produced. Issues around the production of resources cannot be treated as independent from those of distribution, as the two are mutually implicated. This can be illustrated by two examples. The first relates to the distribution of prestige in an imagined scientific community. This community sets up an award process whereby those scientists who are particularly outstanding in their research are bestowed titles and treated with great respect by members of the community. The mechanism operates successfully at first, distributing the group's prestige to those generally accepted as the most insightful. However, at some point, it is co-opted by a sub-group of the community, whom we can consider a Barnesian 'status group'. This sub-group finds a way to control the committee which bestows the awards, so that members of the sub-group receive titles even when their work is below the standards of other scientists. In other words, this status group has found a way to monopolise the 'prestige' resources of the community.

When analysing such a development, it is crucial not to focus only on the activities of the status group and the immediate distributional effects. The attempt to monopolise the prestige resources of the community will have a downstream effect upon the production and distribution of those resources themselves. Once it becomes generally recognised that titles are no longer being handed out on the basis of research excellence, title holders will no longer be treated with honour by community members, as the title no longer represents the general approbation of the community. The 'resource' of prestige stops being produced by

the community, and what is left to be distributed by the awards process is nothing more than the self-valuation of members of the sub-group.

The point of this example is that the 'interests' of a status group are not self-validating. These interests rely upon the monopolisation of resources produced elsewhere, and the attempt to realise such interests may well have an effect upon the production and distribution of such resources. Another way of putting this is that status-group members may be wrong in their perception of their interests. They may believe that the monopolisation of resources is in their interests, but the attempt to do so leads to the non-production of the resources that were valued in the first place. As a result, the conception of interests held by a status group is not self-validating; the interests of the status group are bound up with wider processes of the production and distribution of resources.

These questions of production and distribution are particularly salient when we consider economic resources. Barnes certainly acknowledges that these resources have to be produced through institutional structures and practices, those within capitalism including private property and alienable labour as Marx emphasised (Barnes, 1995: 190-2). However, he views the character of the institutions making up capitalism as a 'gigantic self-fulfilling prophecy' which exist only because actors treat them as if they exist in order to calculatedly achieve their goals (Barnes, 1995: 191). This point is also emphasised by Barnes in his article 'Social Life as Bootstrapped Induction'. He suggests that if capitalism is *believed* to be stable by actors, then this helps to 'constitute that stability'. Likewise, if actors would only start to think a socialist society into existence, then such a society could emerge. As Barnes states: 'If everyone would treat society as a different pattern, then it would be a different pattern' (Barnes, 1983: 536).

It is undoubtedly true that if actors started to believe and act in different ways, then the character of society would change. However, what cannot be guaranteed, in either a capitalist or socialist mode of production, is that the resources desired by actors will actually be produced. To believe that capitalism will meet the needs of actors is not the same as capitalism being a structure which can in fact meet such demands, and the same goes for socialism. It certainly could have been the case, as Marx argued, that although actors believed that the capitalist mode of production would provide resources for all, its way of organising production was chronically unstable, leading to crises of various kinds. One could agree with Barnes that Marx's predictions about capitalism have not come true, without then agreeing that social institutions have whatever character actors believe them to

have. Marx is correct to suggest that practical, resource generating activity may have consequences for actors and their interests which go beyond current understandings.

This is relevant to the analysis of status groups. It once again suggests that the interests of a group are not self-validating, but have roots in wider social and economic structures. Barnes himself suggests a case where the standard status-group goal of monopolising resources is not in the interests of actors. He argues that, within capitalism, the bourgeoisie give up some of the surplus that accrues to them through the production process, in order to avoid crises of over-production (Barnes, 1995: 190-1). In other words, the bourgeoisie, as a status group, would be mistaken if it defined its self-interest as monopolising as many resources as possible. This is despite the fact that such a conception of interest should, in Barnes' terms, be self-validating. This policy would be mistaken because it would undermine the structures of capitalism by producing crises of under-consumption (over-production), thereby jeopardising profit-making activities in general. The point here is that the structural features of capitalism give the bourgeoisie an interest in certain behaviours which is not self-defined, but is a result of the wider logic of the system.

In this sense, a sociological analysis of the bourgeoisie as a 'class' is more defensible than one that treats them as a 'status group', because the former emphasises that their interests are not self-defined and self-validating. Of course, to locate the interest of a group in a structure is not to claim that such a group will necessarily act in their best interest, as this interest may not be properly grasped by the members involved. It is, however, to stress the Lukacsian point that if they do not act in their best interests, they will have to live with the suboptimal outcomes that result. Furthermore, the problems that result from suboptimal understanding provide a stimulus or tension for groups that motivates a reconception that removes such problems. It is in this sense that having a structural social interest can be said to stimulate action oriented to that interest.

4.5 Conclusion

This chapter has considered the work of Barnes and Bloor as exemplary of a social constructionist approach to science and society. Although their attempt to understand natural scientific investigation as a fully social activity is a step in the right direction, I have suggested that it does not avoid the problems produced by the division between science and society. Those who make this division argue that the variable success of scientific theories can be assessed, and that successful theories are non-social in character. It is then argued that other social practices are not variable in their success, but are adequate on their own

terms. Barnes and Bloor do not transcend this division, but accept the notion of social activity generated by it and apply it to both science and society. This results in two erroneous claims. Firstly, they argue that all institutionalised understandings, whether scientific or otherwise, are self-referential and self-validating. I have suggested that this makes the development of belief unintelligible, and that institutions are variably successful in their relations with the material world. Secondly, they claim that the interests of actors in supporting or challenging an institution are not affected by the success of this institution. I have criticised this, suggesting that actors only have an interest in supporting an institution if it is successful in producing the resources that they are oriented towards.

The weakness of social constructionist analyses of science is not that they illegitimately extend sociological analysis into a realm which is non-social in its constitution. Rather, it is that their conception of the social is an inadequate one, and is as problematic in analysing other social activities as it is in analysing natural science. Only by reconstructing our understanding of sociality to incorporate an analysis of the variable success of institutions can we offer a full and consistent sociological analysis of science and society.

5

Science as Practice: The Actor-Network Approach

5.1 Introduction

In the previous chapter, I argued that the social constructionist analysis of science runs into analytical difficulties because it does not transcend the division of natural science and society. There is no doubt that, from the 1970s onwards, constructionist approaches have been central to the sociological analysis of science. However, since the 1980s, social constructionism has been progressively challenged by analyses that theorise science as a practical activity. The relevance of this development is nicely captured by Pickering in his editor's introduction to *Science as Practice and Culture* (1992). Pickering suggests that constructionist analyses tended to focus on scientific knowledge, analysing it from an epistemologically relativist perspective. This was done to make room for a sociological account in which social forces were held to determine the direction in which knowledge developed. In contrast, when science is considered not as knowledge but as a continuing flow of practical activity, it seems much less plausible to separate the 'social' factors and identify them as the only determinants of what occurs. Instead of being 'socially' constituted, natural scientific practice appears to be a heterogeneous activity constructed of a range of elements (Pickering, 1992: 9). As Pickering puts it in his summary of Ian Hacking's approach:

Scientific culture is made up of all sorts of bits and pieces – material, social, conceptual – that stand in no unitary relation to one another. (Pickering, 1992: 8)

In other words, the direction in which science develops is not determined solely by 'social' factors, but a range of other elements, including the conjunctions of cultural items which allow new lines of inquiry to develop, the character of existing instrumentation, and so on. Furthermore, as a *practical* activity, science is necessarily transformative. The inputs that go into science (social, cultural, material etc.) are altered in the process of activity. Thus the 'social' does not operate externally upon science, but is a contributing element which is reconstructed in the process of investigation.

It is arguable that the large-scale move towards analysing practice was brought about by an increasing interest in studies of technology, which led to the broadening of the sociology of scientific knowledge into science and technology studies. Writers such as Wiebe Bijker, John Law, Donald MacKenzie, Judy Wajcman, Andrew Pickering, Michel Callon and Bruno Latour were instrumental in these developments, publishing many articles of their own, but also editing collections of work analysing scientific and technological practice (for an early collection, see MacKenzie and Wajcman, 1985).

Interesting and important as these studies of technology are, in this chapter I want to focus on the consequences of the shift to practice for the analysis of science. To do so, I shall examine actor-network theory, which rejects the idea that scientific activity is socially determined, in favour of seeing it as part of wider processes of world-making in which both the 'social' and the 'natural' are transformed. Actor-network theory thus attempts to avoid dichotomising the social and the natural, instead seeing them as elements that are altered by processes that order and reorder the world. This approach is also applied beyond the analysis of science to theorise other relations in society.

In this chapter, I will focus on the work of Latour, a founding member of the actor-network school. I shall be arguing that Latour is an insightful critic of social constructionism, and correctly rejects the separation of scientific knowledge and social interests that characterises both constructionism and realism. However, I shall also claim that he fails to offer a coherent alternative to these positions, and that the problems with Latour's account of natural science are transferred across into his philosophy of social science, leading him to unnecessarily limit the role that social science can play in social life.

5.2 Natural science in action

Latour's first notable contribution to the science studies field was *Laboratory Life: The Social Construction of Scientific Facts* (1979), co-written with Steve Woolgar. Woolgar became a prominent exponent of the reflexivist move within science studies, which operated by applying the constructionist principles of the new sociology of scientific knowledge to sociological accounts of science themselves. Latour travelled in quite a different direction, and worked with Michel Callon to develop the actor-network approach, which has been highly influential within the field of science studies. Latour's important writings include *The Pasteurization of France* (1988a [1984]), which combines an extended case study of Pasteurian science and society with a philosophical treatise on the nature of being, and *Science In Action* (1987) and *Pandora's Hope* (1999a), which contain Latour's most detailed

accounts of how scientific activity should be analysed. These works are supplemented by a range of interesting and challenging articles about the problems of existing social science, particularly in relation to technological and natural scientific analysis. I shall start by discussing Latour's general approach to social activity, then look more specifically at how this applies to studies of science. Most of the key aspects of the actor-network mode of analysis are outlined in Callon and Latour's article 'Unscrewing the big Leviathan: how actors macro-structure reality and how sociologists help them to do so' (1981), and it is to this that we shall now turn.

Probably the most fundamental tenet of actor-network theory is that society is constituted by actors struggling to define one another. For Callon and Latour, the structure of society at any given time should be understood as the current state in the ongoing struggle over definition on the part of actors. Society is not a set of relations with predefined characteristics, but an ongoing definitional contest. In actor-network theory, a definition is the way in which an actor uses 'negotiations, intrigues, calculations, acts of persuasion and violence' to become a spokesperson for another actor (Callon and Latour, 1981: 279). If a definition is successfully accomplished, the defining actor has the 'authority to speak or act on behalf of another' (Callon and Latour, 1981: 279). In other words, they can state what characteristics the defined actor has. They can also call on that actor's capacity in their activity; the actor has been enrolled for their purposes. As Callon and Latour put it:

Whenever an actor speaks of 'us', s/he is translating other actors into a single will, of which s/he becomes spirit and spokesman. S/he begins to act for several, no longer for one alone. S/he becomes stronger. S/he grows. (Callon and Latour, 1981: 279)

To the extent that definitions of reality are seen as the 'constructs' of actors, actor-network theory has similarities with constructionist positions such as meaning finitism. Nevertheless, there is a relevant difference between the two approaches. Whereas finitists see definitions as self-referential and self-validating, Latour argues that definitions may fail, in which case the defined actors do not behave as expected (Latour, 1988a, 195-8; see also Callon, 1986). Actor-network theory could thus be said to contain a genuine pragmatic element in which actors' attempts to construct the world may fail to 'work' in practice, leaving them lacking in capacity.

Callon and Latour argue that at an early stage of the definition process few actors have had their characteristics defined, and actors have a basic equality of capacity. However, as time goes on, some actors successfully manage to define the nature of others resulting in differentiation based on asymmetries of capacity. Certain actors have defined the nature of

others for their own purposes; others have been defined. Any particular state is never the final one, however, and competition over definition continues. Offering a biological metaphor, Callon and Latour suggest that we

imagine a body where differentiation is never fully irreversible, where each cell attempts to compel the others to become irreversibly specialized, and where many organs are permanently claiming to be the head of the programme. (Callon and Latour, 1981: 285)

At a point when an actor has successfully defined the characteristics of many others, this actor gains in capacity and influence not because s/he has changed in character, but because of the other capacities that s/he has enrolled and which are working for his/her ends. The power of the actor is not present within the individual but in the joint capacity of those that s/he has successfully defined (Latour, 1986).

The idea that society should be understood as a series of competing definitions on the part of actors is not the only important move made by Callon and Latour. They also argue that one cannot view society simply as a set of definitions of humans achieved by other humans. To do so would be to ignore the important capacity that is generated by actors when they define non-human, material entities, and associate themselves with the power of these entities. Without non-human entities, human attempts to define each other would be easily overturned because of the limited capacities that can be drawn upon to make them stick. In order to make networks durable, more solid components and resources must be drawn upon. Humans manage to create relatively stable networks in which their definitions of others persist, and this is because they also define non-human, material resources which are fairly durable in character. In doing so, they replace 'unsettled alliances' with 'walls and written contracts', and social distinctions are physically marked by 'uniforms and tattoos', in a process which solidifies definitions, and limits their sudden reversibility (Callon and Latour, 1981: 284). Actors thus define networks of human and non-human resources which expand their capacity.

According to Latour and Callon, when society is analysed in this manner, distinguishing between humans and non-humans is an irrelevance. As such, the term 'actant' is coined as a neutral way of referring to what is defined, whether human or non-human. This is a strongly counter-intuitive notion, but needs to be considered in the context of the wider actor-network approach. What Callon and Latour are claiming is that when one takes as a starting point the actor trying to gain capacity through his/her definitions of others, it does not matter whether these others are human or non-human. The only important factor is whether or not the definition is successful or unsuccessful, and whether it is durable or easily subject to

renegotiation in the future (Callon and Latour, 1981: 286). In this sense, it doesn't matter whether an actor is associating with human beings, material elements or anything else. Associations within human societies will typically be 'heterogeneous' in the kind of entities that constitute them, and it is not the specific character of the constituents that is important but their strength and durability. For example, in Callon's study of scallop researchers, the important question is whether both scallops and fishermen can be enrolled into the project in a way that produces the desired outcome for the researchers (Callon, 1986). When we ask whether the researchers have successfully enrolled these different actants we are asking whether the actants will act in line with the desires of the defining researchers.

With these basic principles in mind we can now turn to examine the characteristics of science from an actor-network perspective. Latour challenges the science/society division, arguing that we must not treat 'science' as a special kind of activity that is qualitatively different from the everyday practices of agents within societies. Rather his claim is that scientific activity is constituted by exactly the same processes of definition involved in other forms of conduct. To suggest otherwise, argues Latour, would be to grant science some special, supplementary, definitional force produced by its supposed connection with transcendental reason or nature (see particularly part 2, section 4 of Latour, 1988a). Latour dismisses these kind of arguments as nothing more than magic thinking. Science is not successful because of its mystical, unearthly properties; it gains its success by the standard processes through which capacity is always generated, the association of a range of human and non-human allies.

Although, for Latour, there is nothing that qualitatively distinguishes science from other practices, science is of interest because of its impressive quantitative success at amassing resources. In particular, suggests Latour, science is at the forefront of the 'proof race', or the competition to define actants in society and thus build up resourceful networks (Latour, 1987: 172-3). His goal, then, is to account for this quantitative success without moving outside the basic actor-network presuppositions regarding the nature of all social activity. So how is this success achieved?

Most generally, the advantages gained by science can be attributed to its ability to change the scale of a dispute over definition, so that one side ends up with considerably more actants enrolled, and thus backing it up, than the other. One way that this can occur is through processes of 'collection' which involve 'the mobilisation of anything that can be made to move and shipped back home' (Latour, 1987: 225). Once these elements are mobilised, they can be successfully worked upon by scientists in order to enrol them in their networks. As

an example, Latour suggests that the science of zoology became possible through the standardised collection of samples from around the world that did not disintegrate before they reached the zoologists who wanted to analyse them (Latour, 1987: 225). This, of course, was contingent upon existing networks of travellers with the ability and motivation to traverse the world and collect samples. However, once resources were amassed in one place, the basis was laid for a definitional advantage. Whereas the 'ethnozoologies' of other cultures were limited to those species encountered in their own (more or less) local activities, zoology drew upon resources from many places around the globe. As Latour puts it:

The zoologists in their Natural History Museums, without travelling more than a few hundred metres and opening more than a few dozen drawers, travel through all the continents, climates and periods. (Latour, 1987: 225)

As a result of this mass of materials, zoologists could exercise an advantage in their definitional work, having many elements to compare and contrast all within their reach. This in turn made their definitions of species appear 'general' in contrast to the 'local' ethnozoologists of other cultures. There was no mystery to the start of this science, but merely a change in the scale of activity¹ (Latour, 1987: 225).

Another way in which scientists produce a 'change of scale' is through work in the laboratory. Laboratories are set up so that they can easily and cheaply conduct experiments and tests on various entities. For Latour, such tests are attempts to define the properties of the actants involved, which may be more or less successful. In everyday life, such failures are obvious and public (Latour, 1983: 165). What is special about a laboratory is that it permits the multiplication of attempts at definition so that the possibilities of a successful definition are increased along with knowledge of what modes of definition fail and why (Latour, 1983: 164). Once again, this does not demonstrate something special about the cognitive structures or methodologies of scientists. It is merely an increase in the quantity of definitional activity that can be conducted compared to that of laypersons.

By either relocating resources to a centre, or increasing the number of definitions performed, scientists contribute to the quantitative success of science. However, in both of these cases the extra resources or tests that are gained are not enough in themselves. After all, as Latour points out, a sheer mass of entities or results may overwhelm a few actors trying to manipulate them (Latour, 1987: 233). What is required is some kind of standardised way of dealing with these which makes them less overwhelming, and allows the

¹ Here, as elsewhere (see for example Latour, 1988a), Latour is disputing the idea put forward by epistemologists such as Bachelard that the shift from common sense to science involves a qualitative shift in understanding, that is, an 'epistemological break'.

scientist to 'dominate' them instead of the being dominated by them (Latour, 1987: 226). Latour emphasises that we should think of this as a 'logistical' problem. There is no question of some special 'logical' operation being performed; what is required is the practical manipulation of elements. For Latour, this is where the importance of inscriptions comes in.

In the first place, inscriptions (such as instrument outputs, graphs and tables) allow the summary and classification of many disparate elements. A scientist cannot look directly at hundreds of thousands of samples, or just 'see' the economy by looking out of the window. These can only be 'made visible' by a large scale effort of classification and standardization followed by synopses and summaries (Latour, 1987; 1990). The successful definitions of zoology rely upon such methods resulting in inscriptions that simplify the thousands of samples and make it possible for human actors to grasp and employ them in argument.

Likewise, performing a whole host of experiments is of no use if the results of these are not 'archived, saved, recorded and made easily readable again' (Latour, 1990: 164). When trials are recorded, and one can summarise these inscriptions, the resulting tables and graphs can be used to increase the 'certainty' of one's own definitions and decrease the likelihood of dissent from others. By gathering samples or experimental results scientists have attempted to make themselves strong, but this multiplication of resources must be followed by a recompression so that these can be comprehended and employed by humans engaged in definitional battles. This is where the power of inscriptions comes in. The 'average person on the street' cannot compete with a list of many test results presented by a scientist.

More generally, inscriptions have properties that make them especially suitable for changing the available scale of definitional activity. Because of their portability, stability of character, and ease of combination and comparison, inscriptions can multiply the number of elements that are processed and defined (Latour, 1990). They aid definitional activity by recording outcomes, summarising large numbers of cases, and making new definitions possible by facilitating comparisons and juxtapositions. Inscriptions thus simplify the process of defining actors, and make it logistically manageable. This is the fundamental contribution that they make to the quantitative success of science.

Latour's analysis of the character of science is a rich one, and I have omitted mention of some interesting and important concepts such as that of the black-box or machine² (see Latour, 1987). However, these are based on the same general principles as Latour's other

² A 'black box' is a definition that has become stabilised and can be taken as unproblematic by actors (Latour, 1987: 2-3, 80-3). Such definitions then require a large commitment of energy on the point of challengers to be 'reopened'.

concepts. They contribute to a mode of analysis which examines how definitions are more or less successful, stronger or weaker, and eschew notions such as representational adequacy in place of examining how actors enrol actants for their own purposes.

5.3 Beyond constructionism and realism?

It is now time to consider the contrast between Latour's approach and other accounts of natural scientific knowledge. Latour offers his philosophy with the explicit intention of transcending social constructionist and realist theories, neither of which, he claims, can properly account for the development of natural scientific activity and the transformations this effects on human beings and the natural world. Turning first to social constructionism, Latour agrees with the basic constructionist tenet that the pursuit of scientific knowledge is an interested activity. However, he disagrees with the conception of interests mobilised by constructionists. For constructionists, social interests stand outside of debates, determining which statements are taken to be credible and which are not, and the content of the dispute is an epiphenomenal expression of clashes whose source is elsewhere (Latour, 1999b: 118-9). For Latour, on the other hand, scientific investigation plays an active role in resolving debates and transforms interests themselves. In the case of scientific disputes, actors define non-human actants in order to increase the strength of their claims. As Latour puts it: 'Science is not politics. It is politics by other means' (Latour, 1988a: 229). In other words, for a group to win a scientific dispute they must be able to put into play forces that are not available in 'purely political' situations, that is, situations involving only relations between human beings. Instead, groups build up both human and non-human allies in order to pursue the outcome that they desire. Therefore, contrary to the social constructionist position, the content of the debate is not epiphenomenal but is central to determining who wins an interested struggle. To take the example of the spontaneous generation debate between Pasteur and Pouchet, although each had 'interests' in defending a certain position, these interests could not be realised without backing from non-human actants (Latour, 1987: 84). Pasteur attempted to define micro-organisms as something that did not appear spontaneously, but were an external introduction into a sterile environment; Pouchet tried to enlist micro-organisms onto his side by designing an experiment to show that they could appear in a sterile environment, even when no germs were introduced. Each actor attempted to win the debate by recruiting non-human allies.

Latour also argues that interests themselves may be transformed in the course of scientific practice. Such an idea is antithetical to social constructionism, as the constructionist claim

that knowledge is self-validating means that interests are never undermined or altered by outcomes of scientific experiment or theorizing. As we saw in Chapter 4, Barnes argues that those with an interest in defending a theory can do so consistently no matter what results emerge (Barnes, 1982). Therefore, no result ever has an impact upon the interests of those defending a claim. Latour does not accept such an analysis. Rather, he argues that scientific activity may transform the interests of actors (Latour, 1999a: 122-7). As Latour states:

The congenital weakness of the sociology of science is its propensity to look for obvious stated political motives and interests in one of the only places, the laboratories, where sources of fresh politics as yet unrecognized as such are emerging. (Latour, 1983: 157. Author's emphasis)

That is to say, the definition of new actants in the laboratory brings about shifts in interests and politics that would not occur otherwise. An intriguing example can be drawn from Latour's *The Pasteurization of France*. He argues that Pasteur's successful definition of microbes led to a transformation in the interests of social actors. Reformers called upon the theory of the microbe to argue that it was in the interests of the rich to be concerned with the poor because, to quote Gibier, 'The wretchedness of the poor distills a bitter and virulent bile that reaches as far as the rich man's goblet and contaminates the veins of his children' (cited in Latour, 1988a: 37). In other words, because the health of the poor may effect that of the rich, it was in the interests of the latter to be concerned with the health of the former. Scientific investigation brought about a shift in the interests involved. This can be contrasted with Barnes' treatment of interests in the debate between hereditarians and environmentalists (see Chapter 4). For Barnes, no matter what evidence arose in this debate, the interests of the actors involved were not altered, as their positions could always be defended as adequate on their own terms. In contrast, Latour's analysis of the Pasteur case suggests that the outcome of scientific investigation changed the interests of actors, making new courses of action more rewarding for them. Before Pasteurian science, it may have been in the interests of the rich to ignore the living conditions of the poor; after Pasteurian science it was not.

A further consequence of Latour's emphasis on the enrolment of non-human actants is that the symmetry principle defended by social constructionists such as Barnes and Bloor must be rejected. For the Strong Programmers, every theory that can be institutionalised is equally valid (Barnes, 1976), so belief in the validity or otherwise of a scientific theory must be explained by the social interests that support or challenge it, having no justification outside of this. Such an explanation is 'symmetrical' in that it explains perceptions of validity and lack of validity using the same types of social cause (Bloor, 1991). In contrast,

Latour argues that scientists establish the validity of a theory by mobilising various human and non-human resources, and they attempt to make resilient claims that cannot be dislodged without a greater accumulation of resources on the part of their disputants. Although at the start of a dispute conflicting beliefs may be equally valid (resourceful), as the debate continues, one side will build up more resources than the other, establishing their account as valid (Latour, 1987: 96-100). An explanation of why one belief ends up being taken as more valid than another cannot call symmetrically on social factors because beliefs become more or less plausible by their connection with networks of human and non-human resources that back them up.

This claim has a methodological correlate in Latour's account of how to go about studying science. Instead of starting from the assumption that beliefs are always equally valid and analysing them symmetrically, one must follow the practical activity through which beliefs are made more or less valid. When a dispute is open and neither side can definitively establish their claims, then the sociologist can note the equal validity of claims at this point. However, when one side has closed the debate by amassing enough resources to make their account the only plausible one, it does not make sense for analysts to treat these beliefs as equally valid. To assert equal validity at this point would be to come up against a powerful scientific account with nothing but a few epistemological arguments to hand, a battle which Latour suggests sociologists are always likely to lose (Latour, 1987: 94-100).

Of course, those who subscribe to a social constructionist position have not simply capitulated in the face of Latour's attack. Probably the key constructionist response has been to criticise Latour's suggestion that non-human actants have a role in explaining why scientific debates are resolved in one way rather than another. Constructionists argue that every time Latour tries to specify how a non-human actant contributes to the resolution of the debate, this account can be demonstrated to be a social construction (Schaffer, 1991; Collins and Yearley, 1992a). In other words, an account of the characteristics of a non-human actant is only one perspective, defended by a specific social group; another group with different social interests could present an equally defensible account of the actant which contradicts the first. By not recognising this, suggests Schaffer, Latour tacitly sides with the winners of the debate who were able to establish their (challengeable) theory as representative of reality. So, for example, when Latour suggests that microbes enthusiastically behaved as Pasteur wanted, he fails to acknowledge that there was a lengthy controversy between Pasteur and Koch over the characteristics of microbes (Schaffer, 1991: 187). Schaffer, along with Collins and Yearley, argues that the only viable approach to

scientific disagreement is to symmetrically analyse the social motivations behind different accounts of actants, and show how the ultimate acceptance of one account over another is a social process (Schaffer, 1991: 189-90; Collins and Yearley, 1992a). Contrary to Latour, actants cannot be said to play an independent role in the development of knowledge.

These responses are essentially a restatement of the social constructionist position. As such, it is not clear that they successfully deal with Latour's most damaging criticisms of constructionism. Firstly, they do not respond to his argument that the notion of a purely 'social' interest or influence is a dubious one. As I argued in Chapter 4, it is hard to see what the substance of a social interest can be unless it is related to the successful production of resources. Latour's argument that humans pursue their interests by defining non-human actors to increase their resourcefulness makes a similar point, although I shall argue later that his account of this is not unproblematic. The constructionist claim that the content of scientific theories can be explained using social factors does not go any way to explaining the character or force that these factors have independent of successful truck with the natural environment.

Furthermore, the relevance of non-human actants seems to be particularly strong in relation to the transformative aspects of scientific activity. As we have seen, Latour argues that the successful definition of new actants can change the interests of social actors. The idea that shifts in interests might be connected to new capacities in dealing with nature at least provides a basis for explaining how interests change. It is not clear that social constructionists can offer a coherent account of why or how interests change, due to their separation of interests from issues of resourceful interaction with the world (see Chapter 4 for further discussion). Given these points, it is not clear that constructionism provides a sound basis for sociological analysis.

Having considered Latour's criticisms of constructionism, it is now time to turn to his critical appraisal of a realist analysis of science³. Latour's objections to realism centre on two points. Firstly, he claims that realists wish to exclude the influence of socially interested behaviour upon scientific knowledge. They argue that genuine science bears the impress of nature alone, and human's interested behaviour can only be a disruption to this. Realists wish science to grasp the 'objective object' and produce knowledge that has 'no human origin, no trace of humanity' (Latour, 1999a: 13). They believe that genuine science can tell

³ It should be emphasised that Latour is analysing realist construals of science, not of social life more generally.

us once and for all whether an object does or does not exist, producing knowledge that, because of its grounding in nature, is beyond revision (Latour, 1999a: 155-7).

Secondly, and connectedly, Latour argues that realists have a static and unhistorical conception of nature. One needs to be careful to avoid misinterpretation here. As Latour points out, realism can cope with the evolution of nature in terms of the development of living species and other processes that involve change over time (Latour, 1999a: 146). He suggests, nevertheless, that realism cannot deal with the historicity of nature in relation to human interactions with it. Realists consider that the properties of nature are not transformed by human influence. If, for example, there is a shift in scientific argument from viewing spontaneous generation as feasible, to regarding it as impossible, realists do not suggest that nature itself has changed (Latour, 1999a: 155). Rather they suggest that it was true all along that spontaneous generation did not exist, and it is just representations of nature that have changed. Realists thus split epistemology and ontology, arguing that human representations of nature may change (have a history) but nature does not (Latour, 1999a: 146).

As we have seen in this thesis, there are two main strands of realism, epistemological and ontological, which offer different conceptions of natural scientific activity. A moment's pause is necessary to establish whether Latour's arguments apply to ontological realists, epistemological realists, or both, as he does not make this distinction himself. In relation to Latour's claim that realism sees science as bearing the impress of nature alone, this definitely applies to epistemological realism. Although epistemological realists would argue that understandings are theoretically mediated, they nevertheless wish to infer from the success of a theory to a claim that it provides a description of the world that is (at least partially) true. Genuine science is thus marked by its correspondence to reality. Likewise, mature science can legislate with a great degree of certainty whether an object truly exists or not.

It is not quite so clear that these points apply to ontological realism. Ontological realists do not make an inference from the success of theories to their truth, and argue that theorising always contains a social input as well as that from the natural world. They also emphasise the potential revisability of knowledge in the face of later investigation, and do not make a final judgement about the reality of objects or structures. Although this may appear to exempt ontological realism from Latour's criticism, I would argue that this exemption cannot be complete. O-realists argue that knowledge refers to ontological structures, and see the input of nature as the causal influence of these structures. It would seem, then, that the ideal state pictured by ontological realists is that in which scientific knowledge successfully

corresponds to the structures in the natural world, and thus bears the impress of nature alone. Although the social is admitted to be a factor, it is hard to see how this can operate as anything but a blockage to such knowledge. Neither is the question of the finality or otherwise of knowledge clear-cut in ontological realism. Although O-realists admit that accounts may always be revised in the future, I argued in Chapter 1 that their postulation of a gap between the real and the actual is used to justify the preservation of accounts in the face of evidence that contradicts them. Thus, reconstruction is admitted as a possibility, but O-realism contains a get-out clause that can be used to protect accounts of structures indefinitely, and give them an air of finality.

Turning to Latour's criticism of static conceptions of nature, this straightforwardly applies to both epistemological and ontological realism. Both philosophies argue that representations of nature have a history, but that nature does not, in Latour's terms. Neither would accept that if a phenomenon was held by scientists to exist for some period of time, it should therefore be granted ontological status even if it is now held to be non-existent. They would thus accept the separation of epistemology and ontology⁴.

Having clarified the nature of Latour's criticisms, it is now time to consider his positive alternative more fully. The essence of Latour's position is that both human beings and nature are altered in the process of scientific activity. In relation to the social realm, actors are transformed, as are interests, by the definition of non-human actants in science. As to the natural world, the very ontological status of objects is altered in the course of scientific investigation. What Latour means by this is best grasped by looking at his discussion of Pasteur and the lactic acid ferment (Latour, 1999a, Chapters 4 and 5).

Pasteur's contribution to understandings of fermentation was to argue that a 'specific micro-organism could explain fermentation', something he attempted to demonstrate in relation to lactic fermentation (Latour, 1999a: 116). Prior to Pasteur's important paper on the subject, it was not known what caused lactic acid fermentation, although it was believed to be a purely chemical process. Pasteur argued that a yeast played the principle role, and thus that the process was not just chemical but had a biological input. His laboratory work was dedicated to defining the yeast and its properties. Pasteur was then able to show how swiftly lactic fermentation could be produced using yeast, and demonstrate why previous methods were slow and clumsy by comparison (Latour, 1999a: 121).

⁴ Bhaskar might well criticise epistemological realism for failing to separate ontology and epistemology properly, and for accepting a Humean account of laws. However, this is an issue that does not need to be dealt with here.

Latour argues that this experimental process and its reception transformed both humans and nature. The transformation of human beings occurred on the level of individuals and also in the organisation of knowledge production. In relation to individuals, Pasteur changed from being secondary in the field, behind important scientists such as Liebig and Berzelius, to being a central figure who undermined the chemical theory of fermentation (Latour, 1999a: 117). Pasteur's successful definition of a new actant changed his location in the scientific hierarchy of this field. Looking at broader shifts, the organisation of human knowledge-producing activities is altered as an outcome of the scientific activity. Pasteur's work made the specialism of biochemistry possible, meaning that scientists no longer engaged in only biology *or* chemistry (Latour, 1999a: 144). A new disciplinary configuration emerged to deal with the new kind of actant that had been defined.

If these are the transformations that take place among human beings, what are the transformations of nature that occur? Latour argues that one can trace the existence of the yeast and its changing 'ontological status' throughout the course of Pasteur's scientific activity (Latour, 1999a: 116-122). Initially, Pasteur did not describe the yeast as a tangible, fully-constituted phenomenon, but instead listed a set of low-level sense-data (colours and shapes) which described aspects of the process of lactic fermentation. At this stage, the yeast was nothing more than 'a cloud of transient perceptions' (Latour, 1999a: 118). However, as Pasteur continued his work, the yeast became more clearly defined as a particular bounded actor. The first step in this direction was to list a set of actions or 'performances' that could be witnessed in the laboratory, for instance the triggering of fermentation, formation of crystals, and so on (Latour, 1999a: 119). These performances were then attributed to a substance, yeast, which was understood to be their origin, and which expressed its nature through them (Latour, 1999a: 120). Thus, at the end of the process, yeast became a 'full-blown independent entity' that had a specific kind of influence upon events, being the cause of lactic fermentation.

It is fairly clear that this experimental process could be interpreted in different ways. The most typical approach within the philosophy of science would be to argue that Pasteur's work produced a change in the 'representation' of a pre-existent nature. However, Latour rejects such a view, arguing that we should take the shifts brought about by science as alterations of the ontological characteristics of objects themselves. Thus he writes:

...we should be able to say that not only the microbes-for-humans changed in the 1850s, but also the microbes-for-themselves. Their encounter with Pasteur changed them as well. (Latour, 1999a: 146)

This is what Latour means when he says that nature is transformed by scientific activity. Objects can change from scattered perceptions to solid entities with determinate properties, moving between different ontological states. As a consequence, Latour suggests that we should speak of the 'relative existence' or degree of ontological solidity of an entity, rather than asserting its simple existence or non-existence for all time⁵ (Latour, 1999a: 156).

One might summarise Latour's views by saying that both scientists and nature have a history (Latour, 1999a: 150). Science does not simply reveal or discover what already existed in a stable natural realm, but produces new configurations in both society and nature. Latour uses the concept of an 'event' to analyse science's transformative capacity, which is contrasted with a 'zero-sum' model of science (Latour, 1999a: 126). On the zero-sum model, subscribed to by realists and constructionists, scientific investigation does not produce any new outcomes. For realists, experiments reveal nature as it always was; for constructionists, experiments reveal social interests which remain unaltered in the process. Characterising the zero-sum position, Latour states:

Nothing more happens in the history of science than the discovery of what was already there, all along, in nature or society. (Latour, 1999a: 126)

By contrast, Latour's understanding of science as 'event' emphasises that the inputs going into a scientific investigation from nature and society are altered in an unpredictable fashion by it. Both human and non-human actors come out of the process with different characteristics, and the process is not one of discovery of what was always there, but the production of a new configuration. Thus, the history of nature and of society is characterised by fresh and unpredictable developments in both, through their interaction with one another in science.

Having laid out Latour's views, it is now time to assess how successful his move beyond constructionism and realism is. It will be helpful to begin by probing Latour's conception of historical ontology a little more carefully. Ontology refers to the being or existence of things-in-themselves, as Latour acknowledges. However, in order to historicize ontology, Latour assigns it features which seem to belong not to the 'things' in question, but to the subjects who are attempting to know those things. The clearest example of this can be found

⁵ On the basis of these arguments, it is worth noting that Latour does not reinstate a (naïve) realism, as suggested by critics such as Collins and Yearley (1992a) and Gingras (1995). These authors argue that the ascription of influence to non-human actants in actor-network accounts suggests that Latour and others have a realist understanding of the properties of the natural world and its impact upon our knowledge. However, Latour does not justify accounts of actant influence by appealing to their correspondence to reality or success in accounting for ontological structures. Furthermore, his embrace of the idea that ontology can shift over time is completely out of keeping with realism.

in Latour's analysis of the changing ontological status of the lactic ferment (yeast). In the early stages of Pasteur's work, this ferment is characterised only by low-level sense-data relating to colour, shape of formation, and so on. Following a description that emphasises only these features, Latour states:

It would be hard for something to have less existence than that! It is not an object but a cloud of transient perceptions, not yet the predicates of a coherent substance. (Latour, 1999a: 118)

Latour clearly intends this to be a description that applies to ontology. He writes:

It is possible to go from a nonexistent entity to a generic class by passing through stages in which the entity is made of floating sense data, taken as a name of action, and then, finally, turned into a plantlike and organised being with a place within a well-established taxonomy. The circulation of reference does not take us...from one type of trace to the next, but *from one ontological status to the next*. (Latour, 1999a: 122. Author's emphasis)

However, Latour's claims are misplaced. It seems an elementary point to make, but the notions of 'sense data' and 'transient perceptions' do not apply to ontology at all, but describe features of the knowing subject. It makes no difference to entities whether they are analysed in terms of sense-data or have actions predicated of them, as these are aspects of the subjective apprehension of them. Thus, the move from 'transient perception' to 'organised being' is not ontological at all, but a shift in human understanding. Such a move cannot be used to justify a claim that the ontological status of an entity has changed. It does not refer to a change in 'microbes-for-themselves', as Latour suggests (1999a: 146), but simply represents a change in 'microbes-for-humans'.

This is not to say that science never has any impact upon the characteristics of that which it investigates. Probably most notable here are phenomena such as the selective breeding and genetic modification of plants and animals. However, the reconfiguration of these entities is the intelligible outcome of human manipulation of natural processes, unlike Latour's discussion of the changing status of yeast. Human manipulation may result in the generation of new breeds of animals; it cannot result in the shift in an entity from being a 'cloud of transient perceptions' to a 'coherent substance'. It should also be noted that not all science alters the basic characteristics of the entities that it studies, whereas Latour's claims about ontological change apply to all scientific activities.

If Latour is indeed wrong about the history of nature, it is necessary to pinpoint the problems that this generates for the analysis of science. I would suggest that Latour's arguments undermine our ability to intelligibly connect past, present and future science. The key issue here is the alteration of nature within the scientific process. Latour argues that

scientific activity construed as an 'event' does not amount to a 'discovery' of what was already there in nature, but the production of new entities and objects. However, if this is the case, then it must be difficult or impossible to compare the adequacy of past science with present science because each deals with a different set of entities⁶. In Latour's terms, this is not a matter of epistemology and competition between rival understandings, but a break between the past and the present because of their different ontologies.

A comparison with the critical historicist view put forward in this thesis may help to emphasise this point. Critical historicism emphasises the historical character of the scientific process, but sees this as applying to human understandings. To say that understandings are historical is to emphasise that they are always subject to change in the scientific process, and it is never possible to infer either the truth of theoretical claims or the character of ontological structures from theoretical success. Nevertheless, understandings can be compared, and it is possible to make links between past and present science by examining how present theories have resolved difficulties in previous understandings by reconstructing their categories. However, such a comparison could not be made if the present was populated by new entities, and was lacking the old entities with which past theories were concerned. Understandings could not be compared because they would apply to different ontological realms. Latour's claims about changing ontology undermine the possibility of assessing theoretical adequacy, and making intelligible links between the past and present.

This also suggests problems with the actor-network account of the definitional process. As I noted above, the actor-network conception of definition has a pragmatic character in that it emphasises that definitions may fail. However, for Latour, these failures do not have a systematic character, and no conclusions can be drawn from them. It is simply the nature of actants that they may change their character and allegiance⁷ (Latour, 1988a: 195-8). He states:

None of the actants mobilized to secure an alliance stops acting on its own behalf...They each carry on fomenting their own plots, forming their own groups, and serving other masters, wills, and functions. (Latour, 1988a: 197)

⁶ Bloor is equally critical of Latour's elision of understanding and reality (see for example Bloor (forthcoming)). In contrast to the arguments here, however, he wishes to separate the two in order to defend a relativist conception of scientific understanding. On such a conception, the variable adequacy of theories is not an issue.

⁷ Latour states: 'None of the actants mobilized to secure an alliance stops acting on its own behalf...They each carry on fomenting their own plots, forming their own groups, and serving other masters, wills, and functions' (Latour, 1988a: 197).

As a result, Latour places no requirement of consistency between different definitions of actants. This contrasts with the critical historicist approach which insists that failures of definition (understanding) are systematically explicable. They are not the product of the perennially rebellious nature of actants, but of a failure to systematise the world properly. The failure of a definition can then be explained by a better account, not treated as irrelevant for other accounts.

Taking the critical historicist view does not mean rejecting Latour's claims that science is a creative and transformative process. Critical historicism accepts that scientific activity necessitates the creative reconstruction of theories in order to better account for the phenomena that it is trying to grasp (Holmwood, 1996). Science does not simply or directly reveal the character of the world, but is an attempt to improve our theoretically mediated interactions with this world. This process also involves the reconstruction of interests as theories turn out to be more or less successful for their purposes. However, it is not the ontology of the world that changes in this process, but human understandings. By postulating shifts in ontology, Latour undermines an analysis of how humans have creatively reformulated their understandings to remove earlier problems.

Importantly, the critical historicist view also suggests that current scientific understandings are problematic, and it will be necessary to reconstruct their positive and residual categories in order to make further progress (Holmwood, 1996). Future science will not involve the production of new entities (in Latour's sense) which are different-in-themselves from the present stock of the world. Rather, if all goes well, it will have reconstructed the categories of current science and resolved (some of the) problems of understanding within them. Thus critical historicism does not argue that nature is directly revealed in an experiment, nor that a presently successful understanding corresponds with the world, avoiding Latour's charges against realism.

The problems with Latour's account can be highlighted by his own discussion of the relation between past and present science. Latour recognizes that scientists often claim that the entities that they postulate have always existed, rather than seeing them as creations of the scientific process. In order to account for this, Latour argues that scientists attempt to spread their scientific networks of definitions not only through space but through time as well. So, for example, Pasteur worked to reconstruct the past so that both the successes and failures of those previously using fermentation could be explained by his theory (Latour, 1999a: 169). However, as I have argued above, Latour cannot accept this as a demonstration of adequacy, because past and present have different ontologies, and are thus not

epistemologically comparable. Rather, Latour suggests that Pasteur ‘produces’ another version of the past, which is to be placed alongside the version adhered to by the past fermenters themselves. Talking of Pasteur, Latour writes:

He *retrofitted* the past with his own microbiology: the year 1864 that was built *after* 1864 did not have the same components, textures, and associations as the year 1864 produced *during* 1864. (Latour, 1999a: 170. Author’s emphasis)

We thus end up with multiple definitions of any time period in which the events therein are defined differently by the inhabitants of the time, and those who offer later definitions of what was going on at the time. The result of this is that not only does the ontological status of an entity change over time, but it may have more than one status at a particular time. Latour talks, for example, of the enrolment of ‘airborne germs’ by Pasteur in 1864 (Latour, 1999a: 172-3). Having enrolled these entities, Pasteur then proceeded to argue that they had always influenced human affairs, even before his discovery of them. In analysing this, Latour has to capture the definitions of actors of the time, and Pasteur’s redefinitions of the past, both of which are, for Latour, ontological. In answer to the question ‘were there airborne germs before Pasteur?’, Latour thus replies ‘After 1864 airborne germs were there all along’ (Latour, 1999a: 173). Before Pasteur’s discovery, airborne germs both existed (according to Pasteur’s definition) and did not exist (according to the definitions of the actors of the time)⁸.

Instead of accepting that an entity can have more than one ontological status at a time, we should see the difference between present and past theories as one of competition on the level of understanding. Latour is correct to argue that the past is reconstructed by the present, but he refuses to admit that this is simply a difference of accounts in which we can make an assessment of the greater adequacy of one over the other. Part of this seems to stem from a laudable desire to avoid attributing present-day science finality, and we can certainly accept that present day understandings may be revised (Latour, 1999a: 172-3). However, Latour actually employs ahistorical means in order to establish this. Referring to the debate between Pasteur and Pouchet over spontaneous generation, he suggests that future science may decide that Pouchet was right to claim that spontaneous generation exists. This potential for reversals provides the grounds for not judging our present science to be superior to past science. However, a revival of theories of spontaneous generation would not be a

⁸ Latour attempts to make this argument seem plausible by paralleling two questions: ‘Where were microbes before Pasteur?’ and ‘Where was Pasteur before 1822 (the year of his birth)?’ (Latour, 1999a: 173). However, this just serves to heighten the unconvincing nature of Latour’s approach, as if we view the answers as parallel we would state: ‘After 1822 Pasteur was there all along’, which is surely erroneous.

vindication of Pouchet's claims. Pouchet's argument was viable in its context because of specific kinds of evidence that he called on in order to defend spontaneous generation. It was these kinds of evidence that Pasteur's experimental work undermined by pointing to the existence of microbes which generated the phenomena attributed to spontaneous generation. No revival of spontaneous generation could argue for it in the same way that Pouchet did, and any claims for its existence would be necessarily post-Pasteurian, having been pushed forward by the success of Pasteurian science. Thus, the possibility that spontaneous generation will be revived as a theory does not show that we cannot judge whether Pasteur's or Pouchet's arguments were more adequate. It certainly does not necessitate that we see science as a process that affects the ontological status of actants, rather than as an alteration of accounts.

5.4 Disposing with society: the theory of associations

Having examined Latour's account of natural science more closely, it is now time to consider the more general application of actor-network theory. I will recap and expand upon its features, then indicate an important weakness in its account of conflict. The main critical thrust of Latour's approach is directed towards analyses of human activity which attempt to separate off a distinct 'social' realm which has its own internal logic and relations. Unsurprisingly, therefore, Latour strongly criticises Durkheim and the Durkheimian tradition for claiming that the social constitutes a distinct level of reality, the character of which can only be explained on its own terms (Latour, 1999b; Latour, 1992). A paradigmatic example would be Durkheim's claim in *The Rules of Sociological Method* that '*The determining cause of a social fact should be sought among the social facts preceding it...*' (Durkheim, 1938: 110. Author's italics). Latour suggests that such a definition conceives of relations between human beings as having a pure logic of their own outside of the impact of material and natural elements. His jibe at this sort of theorising is that it is a form of 'baboon sociology'. By this, Latour means that such an approach to the study of humans treats them as if their only resources for interacting with one another were those available to baboons, that is, simply those provided by their bodies (See Callon and Latour, 1981; Strum and Latour, 1984). Relations between baboons, and thus the order and hierarchy of a baboon society, are produced using 'in-built' physical and communicational capacities, and alliances and links are forged out of these alone (Callon and Latour, 1981: 281-6). Callon and Latour suggest that it is only this kind of 'society' which could be said to be 'purely social' in its constitution. Human interactions, by contrast, move beyond these parameters. Humans are

not limited to the use of their bodies but employ all kinds of materials in their activities and relations, including tools, uniforms, buildings, written contracts, and so on. This being the case, Latour argues that human activities cannot be understood using categories referring to relations purely between human beings. Rather, analysis of human activity must analyse the links forged between humans and non-human objects. This is not an analysis of 'social' relations but of 'associations' between humans, other humans, and a 'miscellaneous list of extra-somatic resources' (Latour, 1986: 277).

At a meta-theoretical level, the most important aspect of Latour's actor-network theory is its account of how and why networks form. To explain this, Latour calls upon a somewhat abstract notion that I will call the 'will to define'. For Latour, networks of associations are formed by actors who wish to define the world on their own terms, to order the world in a certain way (Latour, 1988a: 166-7). A network of associations is built up by defining human and non-human actants in a way that they will (at least temporarily) accept. By accumulating the support of those who have been successfully enlisted, the claims of the defining actor are strengthened, and they have defined a little more of the world. The support of associated allies can never be relied upon, however, and it is always possible that a competing definer will successfully redefine these allies to support their own system of ordering and equivalence. We should be careful, once again, not to confuse this with a shift in the 'representation' of society. Rather, a change in ordering principle alters the ontological constituents of society itself. It is useful to recall here Callon and Latour's metaphorical account of ordering, in which an initially undifferentiated body comes to have set channels and specialised functions. An ordering principle makes the body as it is, and a competing principle remakes it.

One way of summarising this position would be to say that associational analysis replaces the 'sociology' of human interactions with the 'politics' of human and non-human relations (Strum and Latour, 1984: 17-9; Latour, 1988a). This politics is the struggle of defining actors to construct the world in a way that conforms to their dictates, and to create alliances that order the world as they wish. As Strum and Latour put it:

In our definitions of resources, genes, power, language, capital, and technology, for instance, are all seen as strategic means of enhancing one's influence over others in increasingly more durable ways. Politics is not one realm of action separated from the others. Politics, in our view, is what allows heterogeneous resources to be woven together into a social link that becomes increasingly harder and harder to break. (Strum and Latour, 1984: 19)

These attempts to create durable definitions are frequently struggled over, and there are political moves to dislodge one set of orderings and replace them with an alternative by redefining the actors involved. Whoever wins such definitional contests establishes 'how things are', that is, the characteristics of human and non-human actors.

If actor-network theory is to provide a viable alternative to realism and constructionism, it must resolve the problems inherent in their accounts of social life. One of the difficulties that this thesis has identified with these approaches is their arguments about the 'strategic' or 'interested' notion of the social. Conflict between groups is attributed to differing social interests, but neither constructionism nor realism can coherently explain how these interests arise. This is because their analysis of 'social' interests does not connect them to successful relations with the material world that produce the resources to which interests are oriented.

By dissolving the 'social' as a realm in itself, actor-network theory appears to be a promising alternative to constructionism and realism. However, actor-network theory does not improve on the explanatory ability of these approaches, but exchanges unlocated 'social' conflict for unlocated 'political' conflict. Latour argues that there are disputes over the character of the 'social' as much as the character of the 'natural'. What his approach lacks is an ability to explain the specifics of these disputes, and why an actor or group is pushing for one definition of society rather than another. For Latour, this is simply a product of the will to define. He writes:

...certain forces constantly try to measure rather than be measured and to translate rather than be translated. They wish to act rather than be acted upon. They wish to be stronger than the others. (Latour, 1988a: 167)

This view should be distinguished from the claim that ordering systems grow because they successfully systematise aspects of the material environment. For Latour, that environment is initially undifferentiated, and there is no way to assess whether an ordering system is more or less adequate in relation to it. (Latour, 1988a: 158). Ordering produces an ontology rather than being a reformulation of understandings to better cope with the environment. On this approach, the conflict between forms of ordering has no explanation other than the fact that competing wills exist which wish to order the world in different ways.

Latour's discussion of debates around the nature of society illustrate the problems with his approach (Latour, 1986). For Latour, such debates are actually attempts to redefine the form of society, to create new associations and configurations. Theories that try to establish the nature of the basic units of society are actually engaged in recomposing it.

Latour states:

It is clear, for instance, that if the units are two classes engaged in a constant struggle whose form is defined and counted in terms of the use of labour value then society is made to move in one direction: some members will be defined by others as parasitic exploiters who hold great power. (Latour, 1986: 271)

In other words, debates over society's real character lead to its reconfiguration. The vehemence of these debates comes from the 'entire range of groups dissatisfied with the genealogy of their positions', providing the impetus for a definitional struggle (Latour, 1987: 270).

It is important to ask what tools Latour has to explain these struggles. Firstly, we can enquire why a group might be dissatisfied with its currently defined place in society. A reasonable hypothesis would be that they feel that their characteristics have been misunderstood, and that a better representation of their nature and possibilities is required. However, because Latour eschews epistemology for ontology, he cannot call on a notion of 'misrepresentation'. As a new mode of ordering actually reconstructs the ontological landscape of society on its own terms, rather than altering its representation, there is no sense in which modes of ordering can be compared for their adequacy. Rather, Latour can only call on the existence of an abstract will to (re)define in order to 'explain' the dissatisfaction of a group.

The inadequacy of this is particularly apparent in relation to Latour's discussion of the Marxist 'redefinition of society'. In a footnote on this issue, Latour states:

It is never sufficiently emphasised that Marxism is in effect a mode of calculating all the exchanges practised in a society. If labour value is used as a standard, then the same capitalist who appeared to pay for everything at its price when counted in exchange value, appears as an exploiter. The indignation of the exploited is maintained as long as the accounting system is enforced. If all the exchanges in a society are now counted in *kilocalories* a quite different list of the exploited and parasites is drawn up. (Latour, 1986: 278, n. 6. Author's emphasis)

For Latour, the Marxist mode of calculation is just one of many that could be used, and its attempt to divide society into the bourgeoisie and the proletariat is equally contingent in this sense⁹. It seems likely that Latour sees the Marxist definition of society as stemming from the group wishing to be considered the 'proletariat'. A problem with this approach is to explain why any social group would wish to define themselves as powerless and exploited. Our standard sense is that this would contribute to a transformation of society which did

⁹ Latour's argument that divisions in society are contingent is similar to Barnes' contention that there is no underlying structural logic to interest groups (see Chapter 4 for discussion).

away with the relevant inequality. However, on Latour's account, there seems to be no reason why a social group shouldn't skip the stage where they define themselves as 'exploited' and just define all groups as equally empowered. The claim to be exploited only really makes sense if this is intended to offer a way of ordering society that is not simply *different* to the present one, but to demonstrate the *problems* with the existing system for the purpose of moving to an alternative that removes those problems. Thus, the Marxist claim is that the emergence of an immiserated social group demonstrates problems with the bourgeois order that is inscribed in capitalist institutions, in that an order dedicated to freedom generates the unfreedom of poverty. Likewise, labour value is not intended to be merely a different way of measuring exchanges than that provided by exchange value; rather, it is intended to offer a better 'ordering' of activity than that provided by the concept of exchange value. These Marxist conceptions are intended to indicate problems with the existing order and its self-understanding, and contribute to their replacement by more adequate alternatives.

In cases such as this, Latour's abstract notion of the 'will to order' offers a poor account of the relation between different ways of ordering the world. It obscures the links between such modes of ordering, and the way in which a new mode is frequently an attempt to resolve problems with an existing one. Whereas Latour sees conflict as a basic feature of social life that requires no further accounting for, a critical historicist approach locates conflict in problems of ordering the material world. It thus theorises how conflict arises from problems of ordering, and examines how conflicts can be resolved by solving these problems. This offers an explanation of the emergence of conflict in specific situations that Latour cannot provide.

Although actor-network theory usefully undermines the idea that there is a 'social' realm that can be theorised outside of human relations with materiality, it does not avoid the problems that emerge in constructionism and realism. For constructionists and realists, conflict can emerge at a 'social' level even when no account of the material basis of such disputes can be given. For example, Chapter 3 considered Archer's argument that there may be 'social' conflicts over resources even when material conditions allow all actors to meet their desired goals. Actor-network theory does not locate conflict any more convincingly by arguing that both social and natural elements are pressed into the service of clashing wills to define. It simply replaces a problematic analysis of conflict on the 'social' level alone with a problematic analysis of conflict referring to both 'social' and 'natural' elements. As with the other doctrines, actor-network theory cannot provide a systematic explanation of conflict and its resolution.

5.5 Social science inaction?

A further aspect of Latour's approach which needs to be considered here is his criticism of social science, which has been a continuing feature of his work. For Latour, the essential problem of the social sciences is that they are engaged in exactly the same practices as other actors: they are trying to define the character of society. What social scientists claim to be 'descriptions' of society are in fact attempts to define actors that reinforce or challenge existing definitions of society in the process. The success of sociologists' accounts does not tell us anything about the nature of society, but is instead the enactment of sociologists' definitions, as with every other actor's 'knowledge' of society (Callon and Latour, 1981: 299). Through processes of surveying, interviewing and investigation, sociologists construct new identities which are part of the generalised struggle over society's character. Referring to the definitional work of the social sciences, Latour writes that 'economies emerge out of economics; societies out of sociologies; cultures out of anthropologies' (Latour, 1999c: 18). Sociology does not describe what is already there, but produces society through its definitions (Callon and Latour, 1981: 298-9). Sociologists fail to recognise this, and mistake their 'definitions' for 'descriptions'.

Latour argues that there is an alternative to the sociological approach. Instead of engaging in activities which define the nature of society, investigators should trace the definitions of other actors. It is for this reason that the subtitle to *Science In Action* is 'How to follow scientists and engineers through society'. In other words, a proper analysis 'follows' the definitions of actors (in this case scientists and engineers), rather than attempting to make new definitions itself. This approach explains the apparently weak conclusion to *Science in Action* which, after six chapters of analytical pyrotechnics, concludes with Latour's statement that he just wishes to clear a 'tiny breathing space' for 'those who want to study independently the extensions of these [scientific] networks' (Latour, 1987: 257). Critics such as Steven Shapin have found this to be more like a damp squib than a spectacular closing barrage (Shapin, 1988: 542-3). However, it is important to see that this remark is utterly in keeping with Latour's general orientation. Latour is attempting to avoid a mode of analysis that is constructive and definitional, opting instead for the goal of recording the definitions and associations of others. Any 'stronger' version of analysis would be problematic for two reasons. Firstly, the analyst would be 'defining' actors rather than describing them. Secondly, by getting involved in struggles of definition, sociologists would be displaying the will to order the world.

Rejecting such a role for sociology, Latour writes:

Providing an explanation is, in a nutshell, working at empire-building; the more powerful an explanation, the larger the empire and the stronger the material in which it is built. What we admire in powerful theories we should also admire in freeways, multinational corporations, satellite networks, weapon systems, international banking and data banks. If we do not admire these achievements, there is no basis for using a double standard and letting the 'powerful theories' stand apart and alone be worshipped. (Latour, 1988c: 162)

Powerful explanations are merely another expression of the will to order¹⁰. Sociology should not seek such explanations, but follow the orderings of other actors in society.

Latour has developed this criticism in his most recent work by arguing that sociologists who attempt to discover the true nature of society have 'legislative' pretensions (Latour, 1999c; Latour 2000). The search for structures of society unknown to actors is argued to be an attempt to circumvent due political process. If sociology can discover the underlying nature of society which is hidden to actors, then policy decisions can be taken without political discussions of which ends and means are desirable. When scientific knowledge is available, the views of laypeople can be set aside (Latour, 2000: 119). As Latour puts it, with sociological knowledge

it then becomes possible to embark on the huge task of social engineering in order to produce the common good, without having to go through the painstaking labour of composing this commonality through political means. (Latour, 2000: 118)

Actor-network analysis is proposed as the pin to deflate these legislative pretensions. Instead of looking for hidden structures behind activity, it is committed to learning what society is purely by following the definitions of actors (Latour, 1999c: 20). Its job is to 'represent' the definitions that are already made, rather than claiming a better understanding than that of lay actors (Latour, 2000: 119-120).

Although Latour criticises social constructionism in his work, his account of sociology has strong parallels with constructionist approaches. Certainly, his theory differs from constructionism by emphasising that both natural objects and social groups are transformed through processes of definition. Nevertheless, the idea that actors' definitions should not be criticised by sociologists is also a consequence of the Strong Programme approach. By conceiving of actors' beliefs (definitions) as self-validating, the Strong Programmers

¹⁰ As an aside, it is worth noting how undifferentiated Latour's notion of power is. One can imagine being broadly in favour, for instance, of freeways and data banks, without supporting multinational corporations and weapons systems. The size and expanse of an association does not provide an instant guide to its merit or otherwise.

analytically rule out a critical perspective. If beliefs are valid on their own terms, then any attempt at sociological critique must necessarily fail to show problems with them. There is no more to be said about actors' beliefs than what the actors already know. All that sociology can do is to display the content of beliefs and follow their transformations at the hands of interested actors. Both constructionism and actor-network theory rule out the possibility that social scientists could come to understand society better than the actors who create it.

I would argue that these claims are unsustainable. Latour sees sociological knowledge as inspired by a will to order which is in competition with the other ordering forces in society. Where sociology produces convincing analyses, it does so by establishing its definitions over and against others, recreating society in the process. Throughout this chapter I have challenged the notion that processes of knowing and ordering can be understood in this way. Instead of treating ordering as a reconstruction of the ontological environment itself, I have argued that it is better understood as a more or less adequate attempt to deal with that environment. This suggests a possible rationale for sociological analysis that diverges from actors' accounts. Sociological knowledge can show the failings of particular ways of ordering society, and demonstrate how these may be remedied.

This process can be illustrated by considering again Betty Friedan's arguments in *The Feminine Mystique* (1963). Friedan discusses the 'problem that has no name', that is, the depression and listlessness experienced by many American housewives in the late 1950s. Frequently, housewives attributed this to specific problems in relations with family and neighbours, or individual psychological difficulties. If analysis was to follow a Latourian approach, it would simply list these attributions as the definitions of the actors involved. Any attempt to add to these accounts by offering a general structural explanation would be an illegitimate exercise of the sociological will to order. I would suggest, however, that a structural analysis can serve a useful purpose here. By locating the problem in the gendered division of labour and its restriction of possibilities for self-fulfilment, the sociologist attempts to offer a better explanation of the difficulties experienced by actors, and suggests how they might be resolved by a reordering of society. A successful social analysis would not be an external imposition on actors, but provide a more productive solution to their problems than that provided by their current understandings.

By insisting that social scientists *follow* the orderings of actors, Latour makes sure that social science will make no contribution to resolving actors' current difficulties. Actors themselves engage in problem-solving and alter their activities accordingly, and the

Latourian analyst will always be bringing up the rear of such developments, bound to the current understandings of actors, rather than attempting to grasp and resolve the problems inherent in them. Latour commits analysts to 'always coming last' as humans attempt to resolve problems of action in a resourceful manner. Instead, I would suggest that by engaging in problem-solving in relation to current understandings, sociologists can contribute productively to human endeavours without thereby dominating the world which they study.

This still leaves the issue of legislation to be addressed. Is Latour right that the production of social scientific knowledge is intrinsically tied to a legislative attitude? The perception that this is the case is derived from a foundational conception of scientific knowledge in which scientific investigation produces claims that are guaranteed to be true. If such a guarantee existed, then its logical correlate would be that claims about what is best for society could be acted upon without subjecting them to common reflection and discussion. However, a critical historicist approach rejects a foundational notion of science, arguing that the truth of knowledge produced by social science cannot be assumed. This means that social scientific knowledge cannot be straightforwardly acted upon as if it provided an infallible guide to the best way of ordering social institutions. Nevertheless, the alternative to legislation need not be an acceptance that the views of all parties in a dialogue are equally valid. As Holmwood argues, debate over issues of social policy (broadly conceived) is necessary, but it is an *evidential* debate to which social science can submit the results of its substantive investigations (Holmwood, 1996: 121, 132-4). Through research, sociology can hope to improve our understandings of social activity, and demonstrate problems with existing social orders as well as suggesting solutions to them. The validity of this knowledge cannot be assumed, and critical examination and discussion is required to decide which position in a debate is the most adequate. However, sociology can aspire to improve on the understandings already present in society and show how its own theories resolve existing problems. The pursuit of social scientific knowledge, and the production of understandings that are different to those of social actors, does not necessarily reflect a desire to legislate, but to offer convincing evidence for public debate.

Conclusion:

Reconciling Science and Society

This thesis has explored the interconnections between conceptions of science and society offered by various sociological and philosophical theories. It has focused particularly on dualistic approaches, which argue that natural scientific investigation is necessarily different in character to other social activities. Proponents of the dualism suggest that because science has demonstrable successes, it cannot be fully social. Rather, its successes must be based on foundations lying outside of social life. Likewise, dualists suggest that when a practice is fully social, its variable success in achieving goals is not an issue. I would argue that neither of these claims is satisfactory. In this conclusion I want to trace through the arguments offered in this thesis about science and other social practices, in order to show how our understandings of them might be reconciled.

The thesis began by criticising accounts of science generated by the science/society dualism. The key problem with such accounts is that they see aspects of science as non-social. In particular, they attribute the successes of science to its basis upon foundations that are outside of the flux of social change. These kind of arguments were given detailed consideration in Chapter 1. One version of the foundational argument suggests that the empirical categories of successful science are final, requiring no further revision. Logical positivists subscribe to such a claim, arguing that science is based upon an unchanging, unquestionable observation language. However, the chapter also considered more sophisticated versions of the argument, present in epistemological or ontological realism. Epistemological realists argue that the success of scientific categories is evidence of their truth, requiring these categories to be retained in future investigation. Ontological realists suggest that categories may adequately describe structures even when they cannot account for all the events that occur. In all three cases, existing successful categories are reified, suggesting that further scientific investigation will not require their reconstruction. Contrary to this, the chapter suggested that the categories of science are theoretical mediations whose observational truth, theoretical truth or adequacy to ontological structures cannot be inferred from their success. No matter how successful a theory is, its categories may have to be reconstructed to deal with the continuing explanatory problems that provide the subject matter for scientific investigation. The chapter also examined claims that scientific

investigation was founded on rules of reason which guaranteed the adequacy of theories produced. It was argued that such rules do not provide theoretical guarantees, as they themselves are subject to criticism and change over time. Instead of providing solid foundations for scientific investigation, such rules provide the best understandings currently available of which forms of reasoning are reliable and which are not.

Although Chapter 1 criticised foundationalist accounts of the success of science, it argued that meaningful assessments of theoretical validity could still be made. These are not based on external (unchanging) standards, however, but employ the terms of the theories themselves. The key point here is that even though scientific understandings are theoretical constructions, they do not construct the world unproblematically. Rather, they have both positive categories, which are coherent renderings of interactions with the material world, and residual categories, which are those aspects of interactions with the world that are currently inexplicable. The fact that theories have problems on their own terms opens up possibilities of theoretical assessment. A theory can be shown to have improved in adequacy when it expands the scope of its positive categories, and offers a more coherent account of interactions with the world. It may also be possible for one theory to demonstrate its superiority over a competitor by showing that it can offer a coherent account of interactions that are not systematically accounted for by its competitor.

The critical historicist conception of science developed in Chapter 1 was then used to criticise the foundationalist accounts that emerge within sociological theorising. The key move in the work of both Giddens (Chapter 2) and Archer (Chapter 3) is the claim that science is different in character to other social activities. In order to sustain this division, each thinker ultimately suggests that science is non-social in character. Giddens distinguishes between the social and the natural worlds, arguing that the former is constituted by meanings and the latter by objects. However, this leaves natural science in an ambiguous position, because it is a social practice oriented to knowing the natural world. To resolve this ambiguity, Giddens underplays the theoretically mediated character of natural scientific investigation. This is most apparent in his account of natural scientific innovation, which implies that there can be direct knowledge of the object world, rather than seeing such knowledge as a fallible social product. Archer's realist approach is similarly problematic. Archer claims that social life has two levels, one of which contains the scientifically knowable structures of society, and the other of which contains actors' 'social' response to them. By arguing that the analyst can separate structures from their social reception, Archer

implies that they can be known 'non-socially'. Accounts of structure are thus presented as final and fully adequate, rather than as fallible, socially-produced understandings.

The sociological accounts of science considered in Chapters 4 and 5 reject the separation of science and society, and the foundationalism that goes along with it. For social constructionists (Chapter 4) and actor-network theorists (Chapter 5), scientific investigation has the same characteristics as other human activities. However, these approaches give rise to difficulties of their own. Although constructionists such as Barnes and Bloor reject the division of science and society, they do so by subsuming science under the relativist conception of society that was generated by that division. The problems with this understanding of society will be summarised in more detail below. The main issue, however, is that instead of rejecting only foundationalist accounts of success, social constructionism rejects any consideration of the variable success of theories whatsoever. For Barnes and Bloor, theories are valid on their own terms, and can be held to be consistent with any evidence that arises. Contrary to this, the thesis argued that theories typically achieve some consistency, but also generate anomalies that they cannot explain. Whereas consistent understandings allow actors to relate successfully to the environment, anomalies represent unpredictable outcomes that disrupt their expectations. It is thus in the interests of actors to support consistent theories and develop them in order to remove anomalies. As such, the success or otherwise of a theory is relevant to explaining whether actors support it or not.

The actor-network account of science, considered in Chapter 5, initially appears to be a more promising one. Actor-network theorists such as Latour reject the division of science from other social practices, suggesting that science is just one form of the definitional activity which actors engage in to construct the world around them. They also reject the blanket relativism of social constructionist approaches, arguing that definitions may be more or less successful in establishing the characteristics of the world. However, it was argued that Latour's account of definitional processes is a problematic one. Instead of seeing changes in definitions as changes in human understanding, Latour argues that they involve changes to the objects 'in themselves'. This wrongly projects the perceptions and descriptions of human beings onto the realm of ontology. It also makes the variable success of scientific theories impossible to compare, because each theory has its own ontological world.

In relation to the understanding of science, the conclusion of this thesis must be that none of the sociological approaches considered are adequate. I would argue that the critical

historicist approach put forward in this thesis improves upon existing conceptions. Unlike dualistic understandings of science, the critical historicist approach rejects the idea that the success of science is based on non-social foundations. Unlike existing non-dualistic theories, the critical historicist approach offers a defensible account of how the success of scientific theories can be compared. This non-foundational account of the success of science provides the first move towards the reconciliation of science and society. To make the second move, we need to reconsider the accounts of social activity analysed in this thesis.

The most important conceptions of sociality considered were those based on a division between science and society. On such an approach, social activities are said to have special features that distinguish them from the success-oriented approach of natural scientific investigation. The most important of these are the 'meaningful' character of social life, the strategic or 'interested' orientation of actors, and the ability of actors to be reflexive. Issues of meaning are at the forefront of Giddens' work, and, as we saw above, he argues that whereas the social world is constituted by meanings, the natural world is made up of external objects. This has consequences for how the success of beliefs is to be analysed. Firstly, the assessment of social beliefs is not as straightforward as the assessment of natural scientific theories. Analysts must come to *understand* social beliefs, by treating them as 'mutual knowledge' that is successful on its own terms, before these beliefs can be subjected to empirical assessment. The thesis argued against this approach, suggesting that if beliefs can be empirically criticised, then attempts to treat them as successful on their own terms must misrepresent their categories. As an alternative to this, it was suggested that a proper understanding of both scientific and non-scientific categories requires the analyst to see where they are successful (consistent) and where they are unsuccessful (inconsistent).

Secondly, Giddens suggests that the meaningful character of social life allows actors to exercise agency. Unlike interactions with the natural world, social action involves meaningful reflection upon options, with the result that actors have a choice as to their conduct. Contrary to this, the thesis suggested that meaningful reflection is present in both natural science and other social activities. However, this does not introduce choice into matters, as the decisions of both scientific and non-scientific actors are constrained by success. Because scientists wish to interact successfully with the environment, it would be irrational for them not to support the most adequate theory available. Likewise, in order to achieve the goals that they desire, social actors are constrained to draw on meanings that allow them to achieve those goals. To choose to act otherwise would be to choose to be

incompetent. Thus, natural science and social life share a meaningful character, and both are oriented to success in a way that excludes choice.

Like Giddens, Archer divides social activity from science. For Archer, science provides an understanding of the 'objective logic' of situations, that is, the material structures that facilitate or block action. Social actors must take into account this structural logic, but the special feature of social life is actors' ability to produce reflexive and strategic responses to structure. The argument of the thesis was that Archer cannot explain how reflexive or strategic responses benefit actors when they must involve a divergence from objective structural possibilities. Her accounts of the 'reflexive' understanding of social groups show that such understanding blocks the pursuit of their interests rather than facilitating it. Likewise, the conception of strategic action offered by Archer merely contradicts her analysis of the objective possibilities for successful action, rather than showing how strategic divergence from these possibilities can be in the interests of actors. In other words, Archer does not show how actors can gain any benefit from strategic action when it must be contrary to action oriented to a successful interaction with objective structures. As with Giddens, Archer fails to demonstrate that the orientation of social activity is different to that of science.

Although they do not divide science from other social activities, the conception of sociality offered by Barnes and Bloor shares features with the accounts of Giddens and Archer. As noted above, this is because Barnes and Bloor's approach does not transcend the distinction between science and society, but takes a dualistically derived conception of the 'social' realm and applies it to all practices, including science. Thus, the constructionist argument that all institutionalised beliefs are self-validating in character can be paralleled with Giddens' claim that social beliefs must be treated as completely valid 'mutual knowledge'. Unlike Giddens, Barnes and Bloor do not combine this with the contradictory claim that such beliefs can then be empirically criticised. Nevertheless, similar problems emerge with the constructionist explanation of institutional change. Barnes and Bloor suggest that actors may lose faith in institutions, and reconstruct them. However, if institutions really were self-validating they would never generate any problems for actors, and a loss of faith would be inexplicable. As an alternative, the thesis argued that actors' commitment to an institution is based on the success that it achieves. An institution may be reconstructed to improve its success, or abandoned if a more successful alternative is offered.

The other important aspect of Barnes and Bloor's approach is their argument, shared with Archer, that the social has a strategic logic. They argue that actors support institutionalised

beliefs because it is in their interest to do so, rather than because of the success of these beliefs. The thesis suggested that this claim was invalid, and that actors' interests cannot be independent of considerations of the variable success of belief. That is because interested behaviour is oriented to gaining resources, and such resources can only be produced by employing successful beliefs in interactions with the environment. Thus, the pursuit of interests does not clash with the support of successful beliefs. Rather, actors can only achieve their interests, and gain resources, by supporting successful belief. Because they offer no account of how resources are produced, Barnes and Bloor fail to locate the interests that they assign to actors. This is a consequence of leaving considerations of success out of the analysis of social life.

The last approach considered was that of Latour, who rejects approaches that divide science from society, as well as criticising the constructionism of Barnes and Bloor. Nevertheless, his own account of human definitional activity does not locate the commitments of actors any more successfully than these approaches. This is particularly apparent in his analysis of the conflicting definitions of actors. Instead of arguing that conflicts arise where there are problems ordering the material world, Latour suggests that they result from the clash of abstract defining wills. This fails to explain the content of particular disputes, or how they are resolved by finding more successful modes of ordering the world.

From this survey, it must be concluded that existing conceptions of social activity are problematic. The general tendency is to suggest that issues of variable success are not relevant to an understanding of social practices. Instead, the 'meaningful' nature of these practices, and the strategic orientation of actors, is said to differentiate social activities from scientific investigations oriented to a successful relation with the environment. This thesis has disputed such claims. It has argued that both natural scientific investigation and other social practices are meaningfully constituted, and that this does not rule out an assessment of their success. Successes are not achieved through extra-social means, but are the product of the transformation of categories in order to achieve a more coherent understanding of interactions with the world. It has also suggested that dualistic conceptions of strategic action are incoherent, and that action oriented to the pursuit of interests is not in contradiction with action oriented to successful relations with the environment. This thesis thus concludes that a conceptual reconciliation can be achieved by analysing science and other social activities in the same way.

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